I. P. PAVLOV

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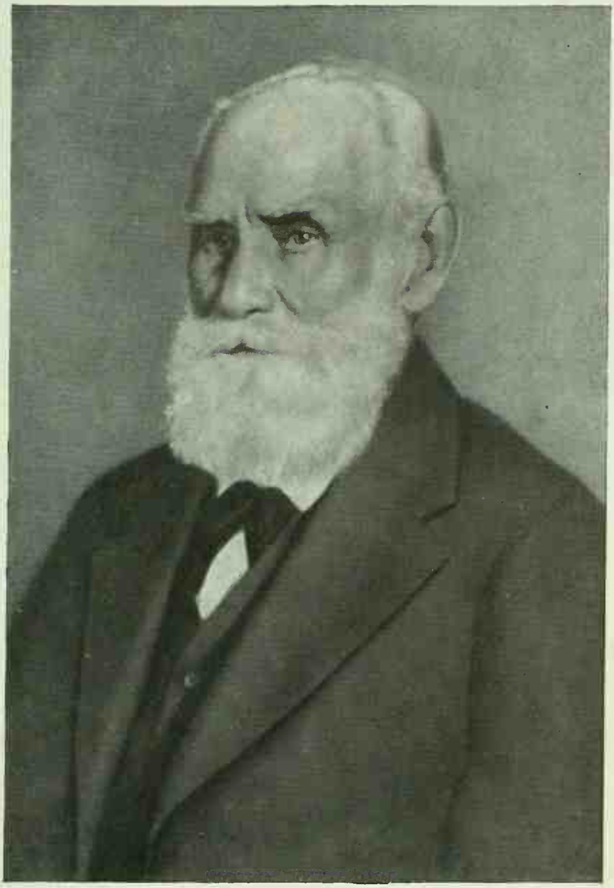

Ив. Павлов

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SELECTED WORKS

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IVAN PETROVICH PAVLOV
AND THE SIGNIFICANCE OF HIS WORKS

> *"Yes, I am glad that, together with Ivan Mikhailovich [Sechenov], I and my group of dear colleagues have won for the mighty realm of physiological research, the animal organism, complete and undivided, instead of a vague half. And this, indisputably, is our Russian contribution to world science and generally to human thought."*
>
> IVAN PAVLOV

A new era in one of the major branches of human knowledge—physiology—is linked with the name of the great physiologist Ivan Petrovich Pavlov.

The wise saying of the ancients "know thyself" has assumed in present-day physiology the form of strictly scientific generalizations of the physiological laws governing the activity of separate organs, systems, and of the organism as a whole in its unity with the environment. The part played by the Russian school of physiologists in advancing physiology, in assuring the tremendous benefit that it brings to vital branches of human practical activity, is truly exceptional.

The names of the great Russian physiologists I. M. Sechenov and I. P. Pavlov stand out like beacons of tremendous power and faithful orientation, lighting up the pathways of scientific progress. J. V. Stalin placed them alongside the names of our greatest men of history and culture, names that are near and dear to the Soviet people.

I. P. Pavlov reconstructed on new foundations such essential branches of physiology as digestion and blood circulation, the theory of the trophic influence of the nervous system; science is indebted to his genius for the founding of the theory of higher nervous activity.

Pavlov followed to the very end—completing a definite phase in the development of Russian science—the toilsome but glorious pathway of searching, blazed with such persistence by the splendid Russian phys-

iologists that preceded him, the path taken by his ideological inspirer and teacher, I. M. Sechenov.

At the same time, stepping out along this pathway, Pavlov, by his investigations and by his passionately irreconcilable attitude towards idealism, carried forward the splendid traditions of the advanced Russian materialistic philosophy—the philosophy which inspired Russian naturalists and above all physiologists in disclosing the secrets of nature and in the bitter conflict with those who admitted the existence of some kind of non-material forces in nature beyond the scope of investigation.

The Soviet country is rightly proud of the Russian physiologists who have made such a big contribution to the development of physiology, as a whole, and to its related branches. No other country has produced so many ardent and uncompromising fighters against idealism in physiology, so many profound and penetrating theorists of this complex science, so many physiologists devoted to the interests of medicine, this noble branch of human practical activity.

The year in which Pavlov was born was the year in which the eminent founder of experimental physiology in Russia, A. M. Filomafitsky (1807-49), died. The work and writings of Filomafitsky, Professor at Moscow University, strikingly illustrate the high level reached by Russian physiology already in the forties of the 19th century. It was in Filomafitsky's laboratory that another remarkable surgeon, V. A. Basov, first performed a stomach fistula operation. This operation was of immense significance for the further study of the physiology of digestion and formed the bedrock for the classical works of Pavlov in this field. At the end of the forties, and the beginning of the fifties, A. N. Orlovsky, a neglected Moscow physiologist and comparative anatomist—a contemporary of A. M. Filomafitsky—carried out experiments jointly with the famous surgeon F. Inozemtsev for the purpose of studying the influence exerted by the nervous system on the nutrition of organisms, i.e., to disclose the so-called trophic influence of the nervous system, to which Pavlov subsequently devoted a number of brilliant works. In his student days at Moscow University I. M. Sechenov was also drawn to the study of the influence exerted by the nervous system on the nutrition of tissues that was carried out by Inozemtsev and Orlovsky; among his early works there is an article on the influence of the nervous system on the nutrition of organs. S. P. Botkin was also a student in the Moscow University at that time; later on, Botkin introduced into clinical medicine the profound physiological theory of the trophic influence of the nervous system.

In his conclusions relating to the trophic influence of the nervous activity Pavlov originally proceeded from his classical works on the nervous regulation of the heart and the cardiac vessels, on the functions of the centrifugal cardiac nerves.

It should be pointed out that the work carried out by Russian physiologists along these lines had already made a big contribution to physiology. On the basis of a special article published by I. T. Glebov, an authoritative Russian physiologist of the fifties of the 19th century, it can be affirmed that the first proof of the existence of a nerve accelerating the work of the heart was adduced by A. N. Orlovsky way back in the early fifties of the last century. Shortly after this discovery by Orlovsky (who was unable to get his works published) two other Russian physiologists, the Cyon brothers, confirmed the existence of this nerve, and this time it won general recognition. One of the brothers, I. F. Cyon, was Pavlov's teacher in the field of experimental technique.

To F. V. Ovsyannikov, another of Pavlov's teachers, belongs the honour of discovering (1871) the so-called vaso-motor centre in the central nervous system. Ovsyannikov's laboratory investigated the trophic influence of the nervous system at the outset of Pavlov's experimental work. Finally, it should be mentioned that influence was also exerted on Pavlov by S. P. Botkin, one of the outstanding representatives of medicine of the 19th century, in whose clinic Pavlov worked. It was in this clinic that his basic idea of the leading role of the nervous system in all physiological processes (the idea of nervism) came into being and matured; and it was here that his views on the connection between physiology and medicine took shape.

Even these fragmentary data relating to the history of Russian physiology testify that the source of the main lines of Pavlov's experimental research can be traced to the works of the Russian physiologists of the period between the forties and seventies of the 19th century, that they are historically connected with them.

Pavlov's theory of conditioned reflexes was a landmark in the development of advanced philosophical thought and natural science in our country, where, as in no other country, the question had been resolutely raised of overcoming the dualism of matter and consciousness, of substantiating the material foundation of the psychical processes on the basis of the unity of matter and spirit, while the idealists affirmed the non-material nature and immortality of the spirit in contradistinction to the material nature and mortality of the body.

In the sixties of the last century D. I. Pisarev, an ardent popularizer of natural science and materialism, carrying forward the materialism and revolutionary democratism of A. I. Herzen, V. G. Belinsky, N. A. Dobrolyubov, and especially of N. G. Chernyshevsky, did much to publicize the highest achievements of the biological science of the time—Darwinism, physiology, etc. Pisarev summoned the young people to a crusade for science, especially natural science; he advanced the profoundly popular revolutionary-democratic task—to use science for the purpose of contributing in every way to the spiritual and social emancipation of the people.

Pisarev's ideas greatly influenced the development of Russian science. K. A. Timiryazev, A. N. Bakh, N. A. Morozov and other outstanding naturalists emphasized its beneficial influence. Pavlov, too, was influenced by it.

In an autobiographical note Pavlov stated: "Influenced by the literature of the sixties, and particularly by Pisarev, our intellectual interests turned to natural science, and many, myself included, decided to take this subject at the University."*

The militant materialistic spirit in Pavlov's works, manifested in raising and solving the problems of higher nervous activity, can be properly appreciated only in its historical association with the traditions of the uncompromising struggle for materialism waged on this crucial sector of the ideological front by the Russian philosophers-materialists, and by their pupil I. M. Sechenov, Pavlov's predecessor and ideological inspirer.

The philosophical writings of Herzen, Pisarev and Chernyshevsky were of enormous significance in moulding the advanced, materialistic traditions of the Russian school of physiology in the fifties and sixties of the 19th century. The ideas of the great Russian physiologists I. M. Sechenov and I. P. Pavlov were also influenced by these works.

The fundamental similarity and the historical and logical link connecting the works of Sechenov and Pavlov consist in that the two men attributed a leading role in the shaping of the highly complex processes of psychical activity to environment, or, as Sechenov expressed it, to the conditions of existence. Pavlov's theory of conditioned reflexes showed that all the diverse manifestations of higher nervous activity are caused by constant interrelations between the organism and its environment, that they arise under certain conditions of the organism's existence. Sechenov's basic postulate that the organism cannot exist without its supporting external environment, is experimentally proved and, in a way, rounded off by Pavlov's theory of conditioned reflexes.

Also typical both of Sechenov and Pavlov is the application of objective physiological methods in studying complex psychical phenomena. Prior to Sechenov and Pavlov all the outstanding explorers of nature proved helpless when it came to the investigation of the so-called spiritual activity; unable to find the way to objective study of it they remained prisoners of philosophical dualism. Sechenov and Pavlov were the first to escape from this captivity, adducing convincing proof of the unity and interdependence of psychical and physical phenomena.

Pavlov's theory of the higher nervous activity rounded off the long searching by Russian philosophers and naturalists who had persevered

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* See present edition, p. 43.

in their efforts to overcome the constant counterpoising of spiritual and physical processes. Their immense labours brought the Russian philosophers and naturalists to the *only true teaching of materialistic philosophy*—the dialectical unity of the physical and the spiritual; and the works of Sechenov and Pavlov furnished irrefutable proof of this teaching. This, so to speak, completed a definite stage in the development of science, the path of diligent searching which led from the philosophical concepts of Radishchev, Belinsky, Herzen and Chernyshevsky to the ideas of the Russian physiologists-materialists of the 19th and 20th centuries.

The historical and logical sequence linking Sechenov's and Pavlov's works is marked by a significant date: two years before his death, in 1903—a memorable year for Russian physiology, Sechenov issued a revised edition of his famous book *Elements of Thought.* This was the last word of the great reformer of the teaching on the nature of consciousness. That same year Pavlov read his first paper on conditioned reflexes at the International Medical Congress in Madrid.

Pavlov stated that Sechenov's *Reflexes of the Brain* exerted enormous influence on him in his youth (in his last years at the Ryazan Seminary), and gave an impulse to his work in the field of the physiology of higher nervous activity, which later developed into the theory of conditioned reflexes. This extremely interesting fact illustrates the complexity of the problems encountered in scientific work, the history of the appearance of varied and important generalizations in science and their links with the generalizations of earlier scientists. The following remarkable excerpt taken from Pavlov's statement describes most convincingly the influence that a genuine teacher exerts on his pupils, testifying to the tremendous effect of a truly scientific book.

"When Tolochinov and I began our investigations the only thing I knew was that the extension of physiological research (in the form of comparative physiology) to the entire animal world would involve, in addition to abandoning our favourite laboratory objects (dogs, cats, rabbits and frogs), abandoning the subjective standpoint and essaying the application of objective methods of investigation and objective terminology (J. Loeb's doctrine of tropism in the animal world and the objective terminology suggested by Beer, Bethe and Uexküll). Indeed it would be difficult and unnatural to think and speak of any thoughts and desires of an amoeba or infusorian. But I believe that in our case, in the study of the dog, man's best friend since prehistoric times, the chief impetus to my decision (although I was not conscious of it at the time) came from the brilliant pamphlet by Ivan Mikhailovich Sechenov, the founder of Russian physiology. It was entitled *Reflexes of the Brain* (1863) and influenced me as a youth. And the influence of ideas which are strong by virtue of their originality and

faithful reflection of reality—especially in one's youth—is profound, lasting, and, it should be added, often concealed. This pamphlet was an attempt, brilliant and truly extraordinary for the time (of course only theoretically, in the form of a physiological outline) to picture our subjective world in a purely physiological aspect.

"At that time Ivan Mikhailovich made an important discovery (concerning central inhibition) which deeply impressed European physiologists and was the first Russian contribution to this essential branch of natural science which had just been greatly advanced by German and French scientists. The strain and the joy of this discovery, together, perhaps, with some other personal emotion, brought about this flowering of Sechenov's thought, which, without any exaggeration, can be described as the thought of genius."*

Thus, we see that Pavlov, the greatest physiologist-materialist of our times, developed and matured on the soil of Russian philosophical thought, and that pre-Pavlovian physiology in Russia paved the way for the tremendous contribution which the great physiologist made.

Pavlov is the outstanding representative of that brilliant galaxy of thinkers, who, in their endeavour to wrest from nature her innermost secrets, always proceeded from strictly scientific experience, from the verification of all scientific discoveries in practice. Physiological experimentation, close contact with clinical medicine, "observation and still more observation," real facts—these were the principles which guided Pavlov, the explorer of one of the most intricate domains of nature. All speculation about natural phenomena without trustworthy experimentation was alien to him.

He wrote: "The more complex the phenomenon (and what can be more complex than life?), the greater the need for experiment. Experiment alone crowns the efforts of medicine, experiment limited only by the natural range of the powers of the human mind. Observation discloses in the animal organism numerous phenomena existing side by side and interconnected now profoundly, now indirectly, or accidentally. Confronted with a multitude of different assumptions the mind must *guess* the real nature of this connection. Experiment, as it were, takes the phenomena in hand, sets in motion now one of them, now another, and thus, by means of artificial, simplified combinations, discovers the actual connection between the phenomena. To put it in another way, observation collects that which nature has to offer, whereas experiment takes from her that which it desires. And the power of biological experimentation is truly colossal. This experimentation has created in the course of some seventy or eighty years practically the entire modern, highly developed physiology of the

* I. P. Pavlov, *Complete Works*, Vol. III, Academy of Sciences of the U.S.S.R., Moscow-Leningrad, 1949, p. 18.

organs of the complex animal. The ordinary educated man, even if he is not yet familiar with biology, upon acquainting himself with the usual, but somewhat more thoroughly arranged course of demonstrative physiology of animals, designed for medical students, would undoubtedly be extremely surprised at discovering the power which the present-day physiologist wields over the complex animal organism. And his surprise would be all the greater upon discovering that this power is the result not of millenniums or centuries, but only of decades."*

The entire scientific activity of Pavlov, spread over a period of almost sixty years, is a brilliant example of experimental investigation of the laws governing the development of living organisms. In the same convincing way he demonstrated the significance of the experimental method for the study of the chemism of digestive processes, for the understanding of the mechanism of the digestive glands, for disclosing the trophic role of the nervous system and the basic laws of the nervous regulation of the work of the cardiovascular system, and finally, for elucidating the complex processes which underlie the phenomena of higher nervous activity in animals.

But Pavlov was not just a continuer of the existing traditions of a strictly scientific, experimental study of organic nature; his experimentation is distinguished for the new and original methods of investigation which he employed. The introduction of new methods of research that elevate the theory of science to a higher level is the typical feature of any classical research worker; and it is this feature that is manifested with particular force in Pavlov's work. Pavlov was a true revolutionary in science; he proclaimed and substantiated the objective, natural method of science in studying the functions of the brain and of the higher nervous activity.

In the third quarter of the last century when he began to investigate the processes of digestion in animals, he set himself the task of elucidating new methods for his investigation. He fully realized that only new methods could provide the key to new theoretical conclusions. "It is often said, and not without reason," he wrote, "that science advances in leaps, depending on the development of experimental methods. With every advance in method, we rise, so to speak, a step higher, and a wider horizon with hitherto imperceptible objects unfolds before us. Our first aim, therefore, was to develop a method"** (1897).

And Pavlov's anticipations were realized. Having found a correct solution to the problems relating to the new methodological approach, and having worked out, as we shall see later, methods of investigation most appropriate to the conditions of the intact organism, Pavlov and his colleagues made a number of major scientific discoveries.

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* See present edition, p. 488.

** *Ibid.*, p. 86.

In the eighteen years that passed between his first description of the method of the isolated "stomach-pouch operation" (1879) and the appearance of his summary work *Lectures on the Work of the Principal Digestive Glands* (1897), Pavlov and his school described a number of fundamental facts relating to the physiology of the digestive glands, thus bringing clarity into the "chaos" which prevailed in this sphere prior to the publication of Pavlov's works. As is known, these works formed the bedrock of the modern concepts of the nervous and chemical regulation of digestion; they gave a clear idea of the sequence of digestive processes in the different parts of the gastrointestinal system; they revealed certain peculiarities of the fermentative processes that take place in the intestinal tract; these works strikingly revealed the dependence of the nature of secretion of various glands on the kind of alimentary stimulus (Pavlov's classical salivation curves); they also laid the foundation for profoundly biological research intc the adaptation of the glands of the digestive system to qualitatively different prolonged nutrition. The works of Pavlov and his school in the field of the physiology of digestion equipped practical medicine with a new and invaluable theoretical weapon.

The Soviet scientist, A. F. Samoilov, now deceased, one of Pavlov's closest disciples, vividly describes the circumstances in which Pavlov realized with complete success his plan for re-equipping physiological research in the sphere of the complex study of the digestive glands. In a tribute on the occasion of Pavlov's 75th birthday, he said:

"I was an eyewitness of the 'stomach-pouch' operation. I remember being fascinated by Ivan Petrovich's courage and belief in the correctness of the plan he had worked out for the operation. The first essays were unsuccessful. About thirty dogs had been sacrificed and much energy and time—almost half a year—had been spent to no purpose. The faint-hearted were already beginning to lose confidence. I recall that some of the professors working in branches of science related to physiology asserted that the operation could not succeed on the grounds that the location of the blood vessels in the stomach made the operation impossible. Ivan Petrovich laughed, as only he could, at these assertions. A few more efforts and success was on the way."*

Consistency, perseverance and passion—these were the qualities which Pavlov bequeathed to the young Soviet scientists in his famous letter to the youth, and it was these qualities that brought him success.

The Pavlovian operative-surgical method of physiology, one of the major achievements of natural science at the end of the 19th century, had its origin in this searching for new methods of physiological

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* A. F. Samoilov, *Selected Articles and Speeches*, Academy of Sciences of the U.S.S.R., Moscow-Leningrad, 1946, p. 98.

investigation. In order to grasp the tremendous significance of this new trend, it suffices to point out that at the time Pavlov was perfecting his operative-surgical method, the so-called vivisectional method, which violated the integrity of the organism, prevailed.

Here is what Pavlov stated in this connection: "It seems to me that in modern physiology a firmer stand is to be taken by the surgical method (I counterpoise it to the purely vivisectional method), which implies performing (skilfully and creatively) more or less complex operations with the aim of either extirpating certain organs, or of ensuring access to physiological phenomena taking place in the depths of the body; of disrupting one or another connection linking the organs, or, on the contrary, establishing a new connection, and so on; it also implies ability subsequently to heal the animal and restore its state, in the measure that the nature of the operation allows, to normalcy.

"I regard the promotion of such surgical technique to be a matter of the greatest importance, because the usual method of simply vivisecting the animal in an acute experiment is, as is now becoming clearer day by day, a major source of errors, since the act of crude violation of the organism is accompanied by a mass of inhibitory influences on the functions of the different organs. The organism as a whole, the realization of the most delicate and most expedient linking of an enormous number of separate parts, cannot, in the nature of things, remain passive to destructive agents; it must, in its own interests, strengthen one part and weaken another, i.e., temporarily leaving aside, so to speak, all other aims, and concentrating on saving whatever can be saved. While this circumstance has been and still is a big obstacle in the way of analytical physiology, it appears to be an insurmountable obstacle to the development of synthetic physiology where it is necessary to determine exactly the true course of one or another physiological phenomenon in an intact and normal organism"* (1897).

It should be stressed that the results achieved by Pavlov in elaborating methods for investigating the digestive glands—methods now used in all physiological establishments—are important because they confirm the great significance of integrated study of the animal organism. This extremely important biological trend in studying the physiological processes in animals that had fully recovered from operations and behaved in a normal manner, a trend that took shape in the course of studying the physiology of the digestive glands, became more pronounced and assumed dominance during Pavlov's work on conditioned reflexes. In a paper which he read before the Ledentsov Society in Moscow in 1910, Pavlov stressed the necessity

* See present edition, pp. 101-02.

of creating better laboratory conditions for his experimental work—conditions that would permit the study of physiological processes in the organism of the animal without damaging its integrity and its normal relations with the surroundings. In this paper he stated:

"There are in addition a number of external influences which have in a greater or lesser degree a destructive effect on the organism. If the fixation of the animal on the stand is connected with very strong pressure on any part of the body, or if the thermal and mechanical apparatus attached to the skin for the purpose of its stimulation (a slight burn or excoriation) damages its integrity, if the introduction of an irritant into the mouth causes an injury of the mucous membrane, even to a minor degree, in all these and similar cases our conditioned reflex will suffer, in a greater or lesser measure, and finally disappear entirely."*

Of particular significance is the fact that the operative-surgical method worked out by Pavlov was regarded by him, in his own terminology, as a method of "physiological thinking." Precisely because of this he was able, at the end of the 19th and at the beginning of the 20th century—the period when analytical physiology reached full bloom—to become one of the few adherents of the integrated study of physiological processes. And it was not fortuitous that he linked the development of the methods of integrated study of physiological processes with the development of synthetical physiology. The subjective experience of Pavlov as a scientist at that time is vividly described by A. F. Samoilov in his reminiscences:

"On one occasion, shortly after I joined the laboratory, I was reading an article in the library of the institute. Ivan Petrovich came in and began to scan the latest magazines. I noticed that something had upset him. He read and re-read the headings of articles and then he exploded: 'We won't get very far if we waste our time on questions such as these!' He threw the magazine on to the table and leaving the room added: 'The very sight of it makes me sick!'

"Greatly puzzled, I immediately picked up the magazine that Ivan Petrovich had thrown away and began to leaf through it. It contained articles devoted to the investigation of separate cells, muscles and nerves; then articles discussed the nature of excitation and conductivity. To me at that time it seemed extremely interesting and valuable. I confess that even now, thirty years later, I am of the same opinion. The general physiology of tissue excitation is fully confirmed and is not in need of any special advocacy. But I think I know why Ivan Petrovich was so scathing and hostile towards the above-mentioned trend in physiological research.

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* I. P. Pavlov, *Complete Works*, Vol. III, pp. 110-11.

"In his view all investigations devoted to separate parts of the body were divorced from the animal mechanism as a whole, from the entire organism; he considered them too abstract and too distracting; in his view, they were not of an urgent character. His talent took him in an altogether different direction, and, fortunately for science, he had the skill and the audacity to brush aside those trends in physiology which were out of his way. This approach enabled him to devote himself more fully to the lines that attracted him most. The sphere in which he was in his element was the animal as a whole and its interrelation with the environment influencing it; and this preoccupation reveals the pronounced biological inclination of Pavlov's talent. Above all he preferred experimentation with intact, non-narcotized animals, which react normally to stimulation and are gay and cheerful."*

Thus we see that Pavlov's work was a striking example of experimental investigation of vital phenomena. He blazed new paths in this direction and equipped physiologists with the method of integrated study of physiological processes. But Pavlov's significance as an experimenter is not confined to this; a distinguishing feature is that he closely connected theoretical analysis of the problems with practice, the problems of physiology with those of medicine. The hours spent with his pupils observing the work of the digestive glands and disclosing the truly majestic picture of the law-governed development of this process, gave Pavlov the naturalist the greatest satisfaction. He wrote: "Indeed, the course of secretion under similar conditions has now become truly stereotyped. The deep impression produced by such, almost physical, precision in a complex vital process, is a pleasant entertainment enjoyed during many hours of observation of the working glands."** In 1899, in a speech dedicated to the memory of S. P. Botkin, Pavlov again pointed to the "strikingly majestic picture" which opens up before the research worker observing the normal course of the digestive process. Pavlov said:

"But should we, as experimenters, be satisfied with this? I think not. When we see deviations from the normal and delve deeply into their mechanism, is it not natural to want to restore them to normal? This and this alone is the only final criterion of the completeness of our physiological knowledge and of our mastery of the subject."*** And as if concentrating his thought on the necessity of establishing the closest contact between theory and practice, of verifying any physiological theory by its practical application in medicine, Pavlov went on to say: "The mechanic completes his apprenticeship by passing a test which consists in assembling the mixed-up parts of the dis-

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* A. F. Samoilov, *Selected Articles and Speeches*, pp. 94-95.

** I. P. Pavlov, *Complete Works*, Vol. II, p. 37.

*** *Ibid.*, p. 354.

mantled machine. This should hold for the physiologist too. Only he who is able to restore the disordered course of life to normal can say that he has acquired real knowledge of life."*

This manifest clarity of purpose, characteristic of Pavlov the experimenter, helped him scientifically to ground experimental therapy and lay the foundations for the Pavlovian tradition in physiology—to study so as to be able to control the process. According to Pavlov, experimental therapy is "in essence the verification of physiology."

Here we come to an essential feature of the teaching of Pavlov, who developed physiology in close connection with practice. Convinced of the great significance of experimentation for the study of the processes developing in a normal organism, Pavlov became a true champion of the experimental method in medicine. "Only by passing through the fire of experiment will medicine as a whole become what it should be, namely, a conscious and, hence, always purposefully acting science. . . . Therefore, I make so bold as to predict that the progress of medicine in this or any other country, in this or any other scientific or educational medical establishment, depends on the attention and care which the experimental branch of medicine enjoys there."** Consequently, it is not accidental that Pavlov's laboratory became an educational centre for the more advanced representatives of medical science who came there to prepare their dissertations. The hundreds of dissertations written in Pavlov's laboratory became valuable contributions to physiology and to experimental pathology and therapy. Pavlov's school produced many leading scientists not only in theoretical physiology, but also in the clinical field. His dream of creating an experimental base for medicine which would satisfy the "craving of men for health and life" (*Pavlov*) has been realized now, in the Soviet period, thanks to the founding of the Institute of Experimental Medicine of the U.S.S.R. Later, there developed out of this Institute, in which Pavlov worked right up until his death, the present U.S.S.R. Academy of Medical Sciences.

Pavlov regarded the correlation between physiological theory and clinical practice as an organic connection of two supplementary aspects: not only physiological experimentation and the conclusions drawn therefrom help to understand the pathological process and the ways of influencing it, but the pathological process, in its turn, is essential for understanding the physiological processes. It was but natural that physiological experimentation brought Pavlov to experimental therapy. Summing up his experimental work in the sphere of studying the digestive glands, Pavlov plainly stated: "This, naturally, brings us to experimental therapy. Discard the practical aim of experimental ther-

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* *Ibid.*

** *Ibid.*, pp. 360, 364.

apy and there remains a new and fruitful method of studying life, because you approach it from a new aspect, and in any case you will be always filling the gaps in the theory of modern physiology."* These conclusions were the result of Pavlov's profound biological comprehension of the normal course and pathology of physiological processes. The pathological process and the normal process are, in Pavlov's view, not dissociated phenomena, they are phenomena of one and the same order.

Throughout Pavlov's scientific activity, observations not only on normal animals, but also on sick human beings and animals, served as an inexhaustible source for his strictly scientific conclusions in physiology. The enormous significance of his observation of diseased organisms in forming the theory of conditioned reflexes, and particularly in achieving comprehension of psychopathological states, is well known. At first Pavlov carried out his observations on casual patients, and then, systematically, in hospital conditions; and he did this with the same consistency and perseverance which characterized his work in the physiological laboratory. Clinical cases stimulated and directed his work of elaborating methods of investigation of physiological processes in the normal organism—methods now regarded as being classical.

For Pavlov a diseased organism was first of all an organism with new relations established between its organs and systems as a result of the illness; and from this point of view he appraised the significance of pathological cases for physiological observations. The organic essence of his entire work was based on the premise clearly expressed in the following proposition: "The domain of pathological phenomena is an infinite series of all kinds of specific—i.e., such as do not occur in the normal course of life—combinations of physiological phenomena. This, undoubtedly, is something like a series of physiological experiments carried out by nature and life, frequently it is a combination of phenomena of a kind that would not have entered the minds of modern physiologists for a long time to come and which sometimes could not even be reproduced deliberately by the technical means of modern physiology. Hence, clinical cases will always be a rich source of new and unexpected physiological facts. It is, therefore, natural for the physiologist to desire a closer union between physiology and medicine."

Pavlov's views on the essence of the profound biological law of development—adaptation—took shape in the course of his work in the sphere of the physiology of blood circulation and digestion.

Already in his early works devoted to the nervous regulation of blood circulation, Pavlov advanced for the first time the idea of the

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* *Ibid.*, p. 354.

reflex adaptation of cardiovascular activity. His work on this subject was published in 1877 under the significant title: "Experimental Data Relating to the Accommodative (adaptive—*Kh. K.*) Mechanism of the Blood Vessels." In our time the vast scientific and practical problem of the reflex regulation of blood circulation under diverse conditions of the vital activity of organisms and of their organs has been elaborated in great detail; the sources of this elaboration can be traced in the works of the Russian physiologists I. F. Cyon, N. O. Kovalevsky, A. S. Dogel, and especially in Pavlov's work.

With the utmost thoroughness Pavlov developed the idea of the adaptive character of physiological phenomena; in doing so he took as his starting-point the numerous observations on the law-governed responses of the glandular cells of the digestive tract to certain natural stimuli acting on the digestive glands (bread, meat, milk), the so-called alimentary stimuli; he also based himself on his research on the adaptation of the fermentative composition of the digestive juices to one or another kind of prolonged diet. Pavlov first approached these problems when he analysed the rich experimental data obtained by his co-worker Yablonsky who proved that the protein ferment units in the pancreatic juice sharply increase under protracted meat feeding—a diet rich in protein—and then gradually diminish when the animal is switched to milk and bread—a diet poor in protein. In this connection Pavlov wrote:

"A more or less stable state of the gland, constantly intensified by prolonging the given diet, could be changed repeatedly in one and the same dog, in one or another direction by means of changing the diet. This circumstance completely precluded the suspicion that in our experiments there took place a certain spontaneous and irrevocable change of the gland as a result of performing an operation or of some other pathological cause."*

But this was not the only thing that interested Pavlov; in appraising the experimental data obtained by his colleagues, he, as a biologist, came to broader conclusions of a general biological character. He stated: "If the diet acts so sharply and powerfully on the chemical nature of the gland, then probably, in the usual natural conditions or under the influence of prolonged (lifetime) domestic routine (as, for example, is often the case with various breeds of dogs), firm and definite types of the pancreatic gland were to be developed. Our experimental data, as we see it, really furnish us with certain indications to this effect. In absolutely identical conditions of feeding, the pancreatic juice of different dogs in our laboratory often differs in respect to content of ferments."**

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* *Ibid.*, p. 54.

** *Ibid.*

We meet with these fundamental ideas of a general biological character, expressed by Pavlov on the basis of the above-mentioned research, in the subsequent long years of his work in the sphere of the physiology of the higher nervous activity. Pavlov chiefly devoted his attention to analysis of the processes which reveal a strict correlation between definite conditions of the environment and definite forms of the organism's reflex activity; at the same time he posed the question of the possibility of developing new, so-called conditioned reflexes in the individual lives of animals, underlying the historical formation of complex phenomena of the animal's adaptation to the surrounding world. Here we come to the definition of the profound theoretical essence of his teaching on conditioned reflexes.

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The history of the theory of conditioned reflexes testifies to the highly intricate and interesting path travelled by Pavlov and his school in the course of shaping this great achievement of natural science of the 20th century.

In a preface to the 5th edition (1932) of his book *Twenty Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals* Pavlov stated: "The present book is the living history of this vast branch of human knowledge, and, I make bold to say, deals with one of the urgent subjects in the elaboration of which this branch is engaged. Here, as in the history of all things, many mistakes have been made, inaccurate observations, wrongly arranged experiments and ill-founded conclusions; but there have also been many edifying instances when much of this was avoided or corrected, and, all in all, there has been a steady accumulation of scientific truth."

Indeed, when we read the *Twenty Years of Objective Study...* article after article and paper after paper, we can clearly picture the conditions of Pavlov's work, the enthusiasm and loyalty of his colleagues, the brilliant manifestations of his thought, his painstaking work on the contradictions which emerged during the elaboration of this complex problem, and the errors and doubts engendered in the process of this work.

The beginning of this great scientific feat was the paper read by Pavlov at the International Medical Congress in Madrid in April 1903. The paper was entitled "Experimental Psychology and Psychopathology in Animals." Typical of the man are the simple and clear opening words of his paper: "Regarding the language of facts as most eloquent, I shall take the liberty of proceeding directly to the experimental material, which gives me the right to speak on the subject of my present communication." The audience assembled at the congress probably anticipated that a paper with the afore-mentioned

title would abound in psychological and psychopathological terminology, as well as in logical propositions, that it would describe cases of pathological activity of the nervous systems of animals under experimental conditions. But Pavlov's paper dealt with altogether different questions, namely, with the results of observations on the work of the salivary glands in the different conditions of physiological experiments. Of course, this paper both in regard to its formulation of the problem and its factual material substantiating highly complex psychological problems, struck the congress as something completely new, and it can be said without any exaggeration that it was like a bolt from the blue.

We shall first dwell on the well-known fact that Pavlov's work in the sphere of the physiology of the higher nervous activity was closely connected with his brilliant cycle of works on the physiology of the digestive glands. A distinguishing feature of the research conducted by Pavlov and his school in the sphere of physiology of digestion was that they investigated with the utmost thoroughness the highly complex problems of the nervous, reflex regulation of the digestive glands. But in the course of this extensive experimental work they came up against the fact that the forms of the nervous regulation of digestive-gland secretion are often conditioned not only by purely physiological factors, but also by factors known as "psychical." The attention of Pavlov and his school was attracted by the fact that reflex influences on the salivary glands exist not only when the alimentary stimuli are in direct contact with the various sensory zones of the animal's digestive tract, but also when the alimentary stimuli are at some distance from the animal and act upon the nervous system not by their primary but secondary properties (by means of signals, according to Pavlov) and, in addition, through the system of sensory (or receiving, according to Pavlov) elements outside the digestive apparatus (eye, ear, skin, etc.). It is interesting to recall that by the term "distant reflexes" or "signalling reflexes" Pavlov originally described the type of reaction which he later termed "conditioned reflexes."

The connection between Pavlov's work in the sphere of the physiology of digestion and his work in the sphere of conditioned reflexes is of a very broad character: they are united not only by their common ideas but also by their common methodological principles. The perfection which Pavlov attained in preparing animals for physiological experiments after well-performed operations, and which enabled him to preserve the integrity of the animal's nervous connections and its normal connections with the environment, was of great significance in disclosing the actual relations in the digestive processes and made possible a new approach to the study of the reflex relations of the organism. It goes without saying that so long as this basic fact was ignored in studying the work of the different glands of the digestive

tract, including the salivary and gastric glands, it was difficult to discern and, what is still more important, to analyse the specific form of the reflexes—the distant reflexes emerging only under definite conditions of the animal's interrelations with the environment.

Pavlov approached this problem after a profound study of the specific forms of secretory activity of the digestive glands which he himself called "psychical secretion." This term was used by him also in his *Lectures on the Work of the Principal Digestive Glands*, published in 1897. In this book Pavlov details the most diverse cases of psychical secretion; however, he did not at that time pose the question of the possibility of analysing this form of secretion, too, as a specific manifestation of reflex activity.

In the second half of the nineties of the last century Pavlov started in earnest his experimental analysis of the essence of "psychical secretion." Although his observations revealed this kind of secretion both in the gastric and salivary glands, he concentrated his attention on the latter. By that time his closest colleague, D. L. Glinsky, had elaborated the splendid method of a constant salivary gland fistula—a method which made it possible to perform successive experiments on the dog's salivary glands over a period of months and even years.*

The very first experiments, carried out by Doctor S. G. Wolfson in accordance with Pavlov's instructions, showed that the mere sight of food was sufficient to obtain a secretion of saliva. The most striking thing in these experiments was th t the quality and quantity of saliva varied depending on what the animals were shown, whether edible or inedible substances. In other words, the salivary secretion caused by the sight of food reproduced, although on a somewhat smaller scale, the salivary secretion which takes place when the mouth is directly irritated by respective substances. Such results were obtained when natural food substances (meat, milk, dry bread, meat-powder) were placed in the mouth or shown to the animal.

Similar experiments were carried out by another of Pavlov's colleagues, Doctor A. T. Snarsky, who obtained very interesting facts. For instance, the repeated introduction into the dog's mouth of acid, which was coloured black, invariably produced profuse salivation. Afterwards, when Snarsky introduced water into the mouth, also coloured black, it produced the same abundant secretion of saliva. A similar effect was obtained when the animal was shown a bottle containing black liquid. The conclusion was quite unexpected for that time: "The black water began to stimulate the glands from a distance only when

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* D. L. Glinsky, *Experiments on the Work of the Salivary Glands* (I. P. Pavlov's paper on them). Proceedings of the Russian Medical Society in St. Petersburg, 1895, 61st year.

coloured (black) acid had been preliminarily introduced into the dog's mouth."

Another experiment consisted in the following: when the dog with constant salivary gland fistulae smelled for the first time in its life the odour of anise oil or of any other odorous substance, it did not react with a salivary secretion. But when, simultaneously with the effect of the odour, the oil was brought into contact with the oral cavity, causing a strong local irritation, the odour alone began subsequently to produce a secretion of saliva.

Snarsky erroneously interpreted the results of these experiments as a manifestation of the animals' specific psychical activity, and he suggested taking into consideration the thoughts, desires and emotions of the animals undergoing the experiment. Discussing with Pavlov the results of his experiments he emphasized the great significance of the dog's inner life and declared that the animal's behaviour was a manifestation of its psychical reaction, that the salivary gland merely reflected a certain inner state of the animal which was hardly accessible to physiological investigation.

These experiments date from the very beginning of the 20th century—Snarsky's dissertation was published in 1901. By then Pavlov had become firmly convinced that it was necessary to replace the concept of psychical secretion with very definite physiological concepts. Hence his heated argument with Snarsky. The latter stubbornly insisted on his subjective anthropomorphic interpretation of phenomena and finally had to leave Pavlov's laboratory.

The more than thirty years' work of Pavlov and his school clearly showed that, apart from inborn reflexes, which rest on the anatomical connection of the central nervous system and of its conductors with the peripheral organs (muscles, glands), there are additional reflexes; the latter may arise in the individual life of the animal as a result of the coincidence of the action of various, to a certain moment indifferent, stimuli coming from the environment, with stimuli that are unconditioned agents of one or another reaction (secretory, motor, etc.). This is the principal theoretical premise for the elaboration of the methods underlying Pavlov's theory of conditioned reflexes, according to which such indifferent agents of alimentary reaction, as light, sound, pricking, etc., become conditioned stimuli of the digestive glands if they coincide in time with the action of the unconditioned alimentary stimulus of the food itself.

From the general biological point of view the experiments carried out by Pavlov's pupil Tsitovich are of special interest. The results obtained by him were published in his dissertation entitled "The Origin and Development of Natural Conditioned Reflexes" and supplied, for the first time, the clearest experimental corroboration of Pavlov's views on the existence of two types of reflexes—inborn, or uncondi-

tioned, and individually acquired, or conditioned. Tsitovich proved that pups with constant salivary gland fistulae, kept on a milk diet for a long period, acquired complex forms of conditioned reflex connections with everything that had a bearing on milk. But the appearance, colour and sounds connected with other foods and the conditions of their feeding, in particular such strong alimentary stimuli as meat and bread, did not evoke conditioned salivary secretion in the animals until they were given a meal of meat or bread. After their first meal of these substances the odour of meat or bread sufficed to evoke a profuse conditioned secretion of saliva.

Pavlov's discovery of conditioned reflexes, his description of new types of the animal's nervous connections with the conditions of life (conditioned reflex connections), represents a great step forward in the development of the theory of reflexes in physiology. Whereas throughout the more than two and a half centuries since Descartes introduced into physiology the concept of reflexes, the reflex had been regarded as the reaction of the animal's organs or of its entire organism to certain stimuli, the Russian physiologists Sechenov and Pavlov, on the basis of anatomically fixed nervous paths, raised and experimentally solved a problem of exceptional significance—the reflex connections of animal organisms bearing an adaptive character and emerging and vanishing during the individual development of the organisms in complete unison with the conditions of existence. Pavlov proved that the reflexes discovered by him are, according to the mechanism of their formation, of a coupling nature, being the result of the coupling of connections between two foci of excitation in the brain, and that they are also temporary, because they vanish under definite conditions.

That the problem could be formulated in this way is due to the Russian school of physiology the founder of which, I. M. Sechenov, as early as 1861, advanced the thesis that "the organism cannot exist without the external environment which supports it, hence, the scientific definition of the organism must also include the environment by which it is influenced."*

Proceeding from this thesis and from the Darwinist interpretation of the laws of development of life, Sechenov asserted that there are in the first place inborn reflexes (effected on the basis of anatomical reflex paths which exist at the moment of birth) and acquired reflexes, elaborated in the course of the individual life experience, and that, in the second place, all the more complex forms of nervous activity are, by the nature of their origin, reflexes.

Sechenov waged a bitter struggle against those physiologists who, unable to comprehend the unity of the organism and the environment,

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* I. M. Sechenov, *Vegetative Processes in Animal Life,* "Medical Herald," No. 26, 1861.

as well as the evolution of the nervous activity, were inclined to endow even the spinal cord with a soul, since they were unable to explain the origin, development and action of the spinal cord reflexes co-ordinated for the given conditions of existence.

That which I. M. Sechenov theoretically substantiated and which he began to elaborate experimentally, was completed by I. P. Pavlov in his theory of conditioned reflexes and in his works on the reflex nature of the cerebral activity.

The reflex theory is, above all, a biological theory. According to Pavlov, the development of a conditioned reflex is first of all a biological process which creates the prerequisite for proper metabolism and exchange of energy between the organism and the external environment. Abundant experimental data revealed to Pavlov the tremendous role played by the nervous system in the basic biological process—in the process of metabolism. Pavlov and his school demonstrated much more convincingly and with greater completeness than anybody before them, that the nervous system plays a leading part in the processes of reception and digestion of food, in its procurement, as well as in the delicate processes of the chemical transformation of the nutritious substances in the organism. The brilliant discovery made by Pavlov is that this continuous process of metabolism and exchange of energy between the organism and the external environment is effected not only by means of a complex of inborn nervous-reflex acts: in the course of the animal's individual development, in each concrete case and situation, there arise new, acquired nervous connections conditioned by the environment (temporary connections, conditioned reflexes) which, in the given conditions, make the interrelations of the animal and the external environment most optimal. In his paper "Natural Science and the Brain" Pavlov defined with the utmost clarity the biological significance of the conditioned reflexes discovered by him. He wrote: "The most essential connection between the animal organism and the surrounding world is that brought about by certain chemical substances which constantly enter into the composition of the given organism, i.e., the food connection. In the lower forms of the animal world it is the direct contact between food and the animal organism or vice versa, which chiefly leads to alimentary metabolism. In the higher forms these relations become more numerous and remote. Now odours, sounds and pictures attract the animals to food substances already in wide regions of the surrounding world. . . . Along with this variety and remoteness, there takes place a substitution of the temporary for the constant connection between the external agents and the organism; first, because, essentially, the remote connections are of a temporary and changeable nature, and, secondly, because, due to their variety and number, they cannot be covered as constant connections, even by the most capacious apparatus. The given food object may be now in

one place, now in another; it may, consequently, be accompanied at one time by certain phenomena, at another time by quite different ones; it may be part of one or another system of the external world, and therefore now these now other natural phenomena must temporarily serve as stimulating agents producing in the organism a positive motor (in the broad sense of this word) reaction to this object."*

Pavlov regarded the conditioned, or temporary, acquired reflexes as an organ of the animal organism especially adapted constantly to effect a more and more perfect equilibration of the organism with the environment—an organ for the appropriate and immediate reaction to most diverse combinations and fluctuations of phenomena in the surrounding world, and to a degree, a special organ for the continuous development of the animal organism. Pavlov said: "The basic functions of the higher part of the central nervous system are the coupling of new and temporary connections between the external phenomena and the work of the different organs, and the decomposing by the organism of the complex of the external environment into its separate elements, that is, functions of coupling and analysing mechanisms.

"By means of these activities there are established finer and more delicate adjustments of the animal organism to the environment, or, in other words, a more complete equilibration of the system of matter and energy which constitute the animal organism, with the matter and energy of the environment."**

The method of conditioned reflexes opened fundamentally new ways for studying the function of the brain, namely, the cerebral cortex and its different functional parts. Pavlov radically revised the views then prevailing on the physiology of the cerebral cortex and based this important branch of physiology on new principles. The old static concepts of the localization of functions in definite, strictly confined sections of the brain were superseded by Pavlov's absolutely original concept of the functions of the cerebral cortex. Of particular significance in this concept is the theory of the so-called analysers. By analysers of the cerebral cortex Pavlov implied the "head end" of the receiving, sensory nerve elements. Pavlov's theory of analysers threw new light on the aims and methods of the physiology of the sense organs as the physiology of the central and peripheral receiving mechanisms, as the physiology of analysers.

The peculiarities of the processes of excitation and inhibition in the cerebral cortex were disclosed and elucidated on an absolutely new basis; Pavlov and his school experimentally proved the applicability to the cortex of the remarkable proposition advanced by N. E. Weden-

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* See present edition, p. 210.

** *Ibid.*, p. 221.

sky, one of I. M. Sechenov's disciples, concerning excitation and inhibition as stages of one and the same process.

Pavlov founded the biological theory of sleep and the remarkable theory of protective inhibition as a physiological method of mobilizing the defensive reactions by means of regulating the processes of excitation and inhibition in the cerebral cortex. In Pavlov's lifetime highly valuable data were obtained on the methods of therapeutic regulation of the processes of excitation and inhibition in the cerebral cortex (by means of bromide and caffeine).

The last fifteen years of Pavlov's life (i.e., from the early twenties) were the best and most fruitful years of his school. By that time the number of his followers had grown considerably; they were able to establish their own, independent laboratories; considerable funds were also allocated by the Soviet Government to the existing Pavlov laboratories for the purpose of extending them and supplying them with better equipment; in addition, the famous Koltushi Biological Station was built specially for Pavlov's research.

The turning-point in all this was the famous decree signed by Lenin in 1921 and providing for all the facilities Pavlov needed for his work. Lenin placed Maxim Gorky at the head of a special commission appointed to carry out a number of measures designed to secure normal conditions for Pavlov in the hard times of the early twenties. The opening lines of the decree noted the enormous historical significance of Pavlov's work for the working people of the world.

Considerable sums were allocated by the Soviet Government for the Koltushi Biological Station; this resulted in the opening of one of the finest biological institutions in the world, where every opportunity was provided for the work of Pavlov and his school. They devoted special attention to problems connected with the evolution of the higher nervous activity. Here, over a period of ten years, they conducted their well-known investigation of the higher nervous activity of anthropoids.*

In 1922 Pavlov's immortal work *Twenty Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals* was published. This was a symposium of articles, papers, lectures and speeches devoted to this important branch of natural science—a branch elaborated by Pavlov and his numerous followers. The book was soon translated into a number of foreign languages.

In the spring of 1924 Pavlov delivered a series of lectures at the Army Medical Academy before a large audience of physicians and naturalists. These lectures summed up the work carried out by Pavlov

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* Prior to this work and parallel with it the problems of conditioned reflex activity of apes were thoroughly studied in the Sukhumi Subtropical branch of the All-Union Institute of Experimental Medicine.

and his school over a period of almost twenty-five years in the sphere of the physiology of the cerebral hemispheres. Before sending them to the printer Pavlov spent more than one and a half years editing his lectures. In 1927 his fundamental book *Lectures on the Work of the Cerebral Hemispheres* appeared in Leningrad; together with *Twenty Years of Objective Study...* this book can be regarded as a major contribution to the development of the natural science of the 20th century.

During the Soviet period new trends have appeared in Pavlov's teaching on conditioned reflexes. A number of his followers have elaborated a new branch of the theory of the higher nervous activity—the comparative physiology of conditioned reflexes; they have disclosed the common and differing features in the formation of conditioned reflexes, and the functions of definite sections of the brain in different animals under the peculiar conditions of their existence (oecological peculiarities).

According to Pavlov's designs, the major problems of the development of conditioned reflexes, in the light of the problems relating to the evolution of functions of the nervous system, were to be elaborated at the biological station founded by him in Koltushi.

There has been opened a new vast field of conditioned reflex connections, which are established on the basis of reflex connections between the internal organs and the cerebral cortex (the work of K. M. Bykov and his colleagues). The extensive application by Soviet physiologists of modern delicate electro-physiological methods in investigating the cerebral cortex must be regarded as a considerable achievement of the objective study of the laws governing the formation of temporary connections.

The theory of conditioned reflexes has been theoretically advanced by Pavlov's followers; it has found wide practical application in analysing the various disturbances of the nervous activity and in elaborating ways and means of restoring it to normal.

The development of the theory of higher nervous activity in the Soviet period is seen in the big contribution made by Pavlov himself, for instance, his broad biological generalizations of the role of conditioned reflexes, his elucidation of the specific properties of the human conditioned-reflex activity, his new principles in the experimental therapy of nervous disturbances, and his vigorous struggle against idealism.

In appraising the significance of conditioned reflexes Pavlov, as a naturalist, invariably took up major problems of general biological significance. For example, when classifying the reflexes, he stated that the inborn reflexes are the reflexes of species, whereas the acquired reflexes are those of the individual. "From the purely practical point of view, we call the first reflex unconditioned and the second—conditioned. It is highly probable (and there are indications to this effect)

that newly formed reflexes, given the same conditions of life in the course of successive generations, invariably become constant reflexes. Consequently, this must be one of the acting mechanisms in the evolution of the animal organism."*

In his last, summary article "The Conditioned Reflex" written for the Big Medical Encyclopaedia in 1935, Pavlov touched on the general biological significance of conditioned reflexes; he pointed out that conditioned reflexes provide all that is required for the well-being of the organism, as well as the species.

In the speech which he delivered at the International Physiological Congress in 1913, Pavlov resolutely stated: "It can be accepted that at a later stage some of the newly formed conditioned reflexes are transformed into unconditioned reflexes by heredity."**

In the early twenties N. P. Studentsov initiated a special investigation in Pavlov's laboratory with the aim of verifying the correctness of this idea. In 1924 the American geneticist Morgan came out against these experiments and their interpretation.

However, Pavlov did not relinquish his elaboration of the problem in this biological direction; on the contrary, he continued it, adhering to his fundamental principle that the conditioned reflex is one of the "acting mechanisms in the evolution of the animal organism," the transforming of individually acquired conditioned reflexes into unconditioned, hereditary ones.

Here begins a new stage in Pavlov's activity—study of the genetics of the higher nervous activity. This new field of research, the bedrock of the work of the Koltushi Biological Station, was designed to give final shape to Pavlov's complex concepts of the biological significance of conditioned reflexes as the basis for the development of inborn (unconditioned) reflexes.

Pavlov and his school elaborated with the utmost thoroughness the typology of the behaviour of different dogs and utilized these observations as a biological base for experiments with different animals and for eventual conclusions in each particular case. In the above-mentioned summary article "The Conditioned Reflex," he pointed out that "the study of conditioned reflexes in numerous dogs gradually led to the idea of different nervous systems in different animals, until, finally, sufficient data were obtained to systematize the nervous systems according to some of their basic properties."

"Thus," wrote Pavlov, "type is a congenital, constitutional form of the nervous activity of the animal—the genotype. But since the animal is exposed from the very day of its birth to the most varied influences of the environment, to which it must inevitably respond

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* See present edition, pp. 221-22.

** I. P. Pavlov, *Complete Works,* Vol. III, p. 217

by definite actions which often become more and more fixed and, finally, established for life, the ultimate nervous activity of the animal (phenotype, character) is an alloy of the characteristics of type and the changes produced by the external environment."*

These Pavlovian ideas laid the foundation for a grand plan for the further investigation of the animal's higher nervous activity by the methods of genetics and physiology and opened up new prospects for this sphere of research. Death prevented Pavlov from accomplishing this task as fully as he had elaborated a number of other branches of physiology—digestion, blood circulation, conditioned reflexes and the trophic role of the nervous system.

Pavlov's theoretical generalizations disclosing the nature of the higher nervous activity culminated in his concept of the first and second signalling systems, of which the latter was regarded by him as inherent only in the human brain.

He said: "When the developing animal world reached the stage of man, an extremely important addition was made to the mechanisms of the nervous activity. In the animal, reality is signalized almost exclusively by stimulations and by the traces they leave in the cerebral hemispheres, which come directly to the special cells of the visual, auditory or other receptors of the organism. This is what we, too, possess as impressions, sensations and notions of the world around us, both the natural and the social—with the exception of the words heard or seen. This is the first system of signals of reality common to man and animals. But speech constitutes a second signalling system of reality which is peculiarly ours, being the signal of the first signals. On the one hand, numerous speech stimulations have removed us from reality, and we must always remember this in order not to distort our attitude to reality. On the other hand, it is precisely speech which has made us human, a subject on which I need not dwell in detail here. However, it cannot be doubted that the fundamental laws governing the activity of the first signalling system must also govern that of the second, because it, too, is activity of the same nervous tissue."**

These exceptionally important ideas were brilliantly expressed by Pavlov in the article "The Conditioned Reflex," written for the Big Medical Encyclopaedia. Before sending the article to the Encyclopaedia Pavlov read it at one of his regular "Wednesdays." According to one of his closest colleagues, those present at the gathering were struck by the depth and originality of the problems raised by Pavlov. Most striking, of course, were his ideas concerning the second signalling system; they opened before his followers new avenues of research

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* See present edition, p. 260.

** *Ibid.*, p. 262.

in the sphere of the physiology of the higher nervous activity, the way indicated to future generations of physiologists by their great teacher. Pavlov's ideas concerning the second signalling system were historically related to the remarkable psycho-physiological views of I. M. Sechenov who advanced the profound problem of object thinking. Sechenov stressed at the same time that abstract thought arises in the course of human interrelations with surrounding objects, and although their verbal expression is sometimes far removed from the original object reality, it is, nevertheless, profoundly connected with it.

The problem raised by Pavlov of the second signalling system and the highly important question of the interrelation between the first and second signalling systems are of exceptional significance for the physiology of the higher nervous activity, as well as for psychology, pedagogics and clinical medicine. At the same time the solution of this problem, to the extent that it concerns man, his words, speech and thought, goes far beyond the bounds of physiology. Pavlov's concepts of two signalling systems in man will, undoubtedly, play an important part in the complex branch of science, the line of development of which was clearly defined by J. V. Stalin's works on the problems of linguistics.

Disclosing the actual laws governing the activity of the brain and the evolution of the higher nervous activity, Pavlov formulated a number of dialectical propositions. This dialectical interpretation of processes was the result of his entire experimental work.

Pavlov's theory of the higher nervous activity is of enormous significance also for philosophy.

S. I. Vavilov, the late President of the U.S.S.R. Academy of Sciences, said: "At all the stages of his scientific work Pavlov unswervingly adhered to the strictly materialistic path and his astonishing results are recognized as a permanent and basic part of the natural-scientific foundations of dialectical materialism."

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V. I. Lenin highly appraised Pavlov's work. The decree signed by V. I. Lenin in January 1921, designed to ensure favourable conditions for Pavlov's work, stressed the enormous significance of his activity for the working people of the world.

That high appraisal testified first of all to the great importance which the Soviet State and the Communist Party attach to advanced science, and in particular, to the branch in which, thanks to Pavlov's genius, it proved possible for the first time in the history of science to apply the precise method of investigation of natural science to the highly complex phenomena of the so-called psychical activity in man and animals. The facts obtained by Pavlov and the generaliza-

tions made by him in the sphere of conditioned reflexes which he had discovered, appeared at the beginning of the 20th century as a new and powerful corroboration of the materialistic view of the unity of mental and physical manifestations, of the physiological foundation of the complex manifestations of behaviour and consciousness. This explains why the teaching was so ardently supported and developed in the young Soviet State which set itself the aim of battling for a new, progressive social system; and this explains why it encountered the animosity of a number of scientists and idealist philosophers, lackeys of the imperialist bourgeoisie.

Among the critics of Pavlov's teaching was Charles Sherrington, leading British physiologist. In London, in a conversation with Pavlov, he said: "You know, your conditioned reflexes would hardly be popular in Britain because of their materialistic flavour." In this way Sherrington openly declared his negative attitude to the theory of conditioned reflexes, which, to him, as a representative of idealistic philosophy, was unacceptable because it was a materialistic theory.

In connection with Sherrington's attitude towards Pavlov's conditioned reflexes a certain interest attaches to the "historical research" of his pupil, the American physiologist John Fulton, who seeks to persuade physiologists all over the world that priority in founding the theory of conditioned reflexes belonged to none other than ... Charles Sherrington! In one of his books on problems of the physiology of the nervous system, Fulton even adduces excerpts and drawings taken from an early work by Sherrington, in an attempt to "substantiate" the priority of the latter in the discovery of conditioned reflexes. In one of my other works I show in detail that the facts refute Fulton's "viewpoint."*

It would be interesting to know what Sherrington himself thought of this gift from the pupil who depicts his teacher as the "founder" of a materialist teaching! At the same time, however, Fulton tries to prove that the theory of conditioned reflexes is merely of historical interest.

It is clear that the above-mentioned statement by Charles Sherrington stems from a definite source—from non-recognition of Pavlov's teaching which "has a materialistic flavour." It is equally clear that Pavlov's teaching is highly appraised by the Soviet Government and the Soviet people precisely because it contributes to the liberation of mankind from the age-old prejudice concerning the dissociation and counterpoising of matter and spirit, of the mental and the physical, a prejudice from which the leading British physiologist could not free himself.

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* Kh. S. Koshtoyants, *Essays on the History of Physiology in Russia*, Moscow-Leningrad 1946, Academy of Sciences of the U.S.S.R., pp. 300-01.

In the second quarter of the 20th century Charles Sherrington again proclaimed that thought, emotions, etc., are not subordinated to matter and to the concept of energy, that they are beyond it, and, consequently, beyond the confines of natural sciences. He also stated that matter and energy are granular, and probably, life too, according to its structure, but not consciousness. The latter, he said, is conceived as a specific phenomenon, which undoubtedly cannot be related to physical energy, etc.

The attack launched by Sherrington on Pavlov's materialist theory of conditioned reflexes, his fundamental idealistic concept of the dissociation of matter and spirit, of the physical and the mental, and his slogan of the unknowableness of psychics—all met with a crushing rebuff from Pavlov. Brought up on the traditions of Russian materialist philosophy and in the spirit of irreconcilability to idealism, and being a true follower of the great thinker I. M. Sechenov, Pavlov, criticizing Sherrington's book *The Brain and Its Mechanism*, at one of his "Wednesdays" in September 1934, said:

"It appears that up to now he is not at all sure whether the brain bears any relation to our mind. A neurologist who has spent his whole life studying the subject is still not sure whether the brain has anything to do with the mind....

"How can it be that at the present time a physiologist should doubt the relation between nervous activity and the mind? This is the result of a purely dualistic concept.... Sherrington is a dualist who resolutely divides his being in two halves: the sinful body and the eternal, immortal soul."*

The same spirit of militant irreconcilability is revealed in Pavlov's struggle against those who tried to prevent science from accomplishing its great aim of getting to know the laws governing the activity of the brain on the basis of natural science. He entered into heated polemics on this subject with scientists in many countries. Highly appraising Pierre Janet as a neuropathologist, Pavlov at the same time criticized him for his erroneous psychological views: "Of course he is an animist, i.e., he believes in a specific substance which is not subject to any laws and which is unknowable."** For over twenty years Pavlov polemized with Claparède, the Swiss psychoneurologist. Denouncing the decorative verbiage of Claparède's psychological reasoning in the sphere of associations and conditioned reflexes, Pavlov called it "sheer twaddle" and added: "Undoubtedly, this is a special breed of people, a special sphere in which there is no place for genuine thought, where it is always buried in the devil knows what."***

* See present edition, p. 563.
** *Ibid.*, p. 609.
*** *Ibid.*, p. 615.

For years Pavlov carried on a heated discussion with the American Lashley. He sharply criticized the so-called Gestalt-psychologists—Woodworth and others.

About Koehler, professor of Berlin University, then investigating the behaviour of animals (apes, in particular), he said the following:

"...Koehler is a confirmed animist, he simply cannot become reconciled to the fact that this soul can be grasped by hand, brought to the laboratory, and that the laws of its functioning can be ascertained on dogs. He does not want to admit this."* Ridiculing the vague descriptions given by Dunker, one of Koehler's pupils, Pavlov called his judgement on the principles of education "thought convulsions."

Thus Pavlov waged a systematic and fierce struggle against those who endeavoured to deflect the solving of a fundamental scientific problem—the essence of consciousness—from the exact path of natural science and to direct it on to the idealistic path (as Pavlov put it, the path of "dualism" and "animism").

Deeply conscious of the tremendous power of the theory of conditioned reflexes as an effective weapon for this decisive sector of the ideological front, the great Soviet physiologist, only three months before his death, addressed his colleagues with the following words (at the "Wednesday" gathering on November 6, 1935):

"We must understand that the conditioned reflexes occupy an exceptional place in the world of physiology because there is a dislike for them on the part of many who have a dualistic world outlook. This is quite obvious. The conditioned reflexes force their way to the forefront. They wage a continuous fight against this dualism which, of course, does not surrender."**

At the Fifteenth International Physiological Congress Pavlov was recognized as the "princeps physiologorum mundi." But among the world physiologists he was also the first tribune, the ardent fighter against idealism. Pavlov never lost confidence in the victory of materialism over idealism. In 1932, in Rome, where reaction and clericalism had long been entrenched, the great Russian materialist-physiologist declared from the rostrum of the International Congress:

"I am convinced that an important stage in the development of human thought is approaching, a stage when the physiological and the psychological, the objective and the subjective, will really merge, when the painful contradiction between our mind and our body and their contraposition will either *actually* be solved or disappear in a natural way."***

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* *Ibid.*, p. 559.
** *Ibid.*, p. 620.
*** *Ibid.*, p. 286.

Pavlov passionately combated idealism, which maintained that the immortal soul and the mortal soma (body) are disunited and fundamentally opposed. Carrying on the traditions of his teacher I. M. Sechenov—the first to proclaim the unity of the organism and conditions of existence—Pavlov founded the theory of conditioned reflex connections, which in his view were indispensable for the maintenance of individual life and for the development of the species. In the course of more than fifty years' work in the sphere of physiology, which enriched this important branch of knowledge with a multitude of new facts and theoretical conclusions, Pavlov took as his starting-point the principle of active intervention in physiological processes. According to him, the aim of the scientist is to "control" physiological phenomena; the physiologist must always bear in mind the interests of practical medicine and experimentally elaborate means for restoring to normal the vital processes deranged by illness.

Pavlov ardently loved his native land, its history, culture, art and science. He was a true patriot of the great Soviet country.

It is these features of scientist and patriot, of the fighter against idealism and obscurantism, innovator and persevering worker, whose life's work was directly linked with urgent practical tasks, and, what is most important, who blazed new trails and opened absolutely new paths in science—it is these qualities that endear Ivan Petrovich Pavlov to Soviet scientists and to the Soviet people as a whole.

Kh. S. Koshtoyants

IVAN PETROVICH PAVLOV

AUTOBIOGRAPHY

I was born in the town of Ryazan in the year 1849 into the family of a priest. I received my secondary education at the local theological seminary, which I recall with gratitude. We had a number of excellent teachers. One of them was the priest Feofilakt Antonovich Orlov, a man of lofty ideals. In general, in the seminary at that time (I do not know how it was afterwards) one could follow one's own intellectual inclinations, which was not the case, regrettably so, in the notorious Tolstoy gymnasiums* (and, I think, also in the present ones). One could lag in a given subject and get on in another, but this did not threaten one with trouble, including expulsion; in point of fact it focused attention on one, and gave rise to speculation about the talents and abilities of the student in question.

Influenced by the literature of the sixties, and particularly by Pisarev, our intellectual interests turned to natural science, and many, myself included, decided to take this subject at the University.

In 1870 I entered the Petersburg University and studied in the natural history section of the physics and mathematics faculty. The faculty was in its heyday at the time. We had a number of professors with great names in science, men who were outstanding as lecturers. I chose animal

* Named after D. Tolstoy, tsarist Minister of Public Education, who converted the gymnasiums into scholastic schools with barrack-like discipline.—*Ed.*

physiology for my major course and took chemistry as a minor. We physiologists were tremendously impressed by Cyon. We were fascinated by his ingeniously simple exposition of the most complex physiological questions and his skill in conducting experiments. One can never forget such a teacher. I did my first physiological work under his tuition.

In 1875, after obtaining the degree of Candidate of Natural Sciences, I enrolled in the third-year course of the Medico-Chirurgical Academy. I did so not for the purpose of becoming a physician, but with the idea that after getting the degree of doctor of medicine, I would qualify for a chair in physiology. I must say, however, that at the time this plan seemed a vain dream because a professorship appeared as something unattainable, incredible.

When I entered the Academy I was to become assistant to Prof. Cyon (he read lectures on physiology at this Academy too) in place of S. I. Chernov, who had to go abroad. But an incredible thing happened: the brilliant physiologist was expelled from the Academy. After some time I obtained a position as assistant to Professor K. N. Ustimovich, lecturer on physiology at the Veterinary Institute. When Professor Ustimovich left the Institute—in 1878 I think—I entered the laboratory attached to Professor S. P. Botkin's clinic, where I worked for many years after taking a course at the Institute for the Perfection of Physicians, and after my subsequent two years' sojourn abroad; I remained there up to the time I obtained a chair. Despite certain unfavourable circumstances in this laboratory, the chief of which were, of course, its scanty means, I consider that the time I spent there was most beneficial for my future in science. First of all I had complete independence and the opportunity to devote myself entirely to laboratory work (I had no duties in the clinic itself). I worked without distinguishing what came within the range of my duties and what within the range of others. For months and years I participated with my entire laboratory work in the researches of my colleagues.

But I constantly profited by this work: I had an ever growing practice in physiological reasoning, in the broad sense of this word, as well as in laboratory technique. Besides, there were the always interesting and instructive (though, unfortunately, very rare) conversations with Sergei Petrovich Botkin. I prepared my thesis on the cardiac nerves in this clinic, and it was there, upon returning from abroad, that I began my research into digestion, which later won me considerable fame abroad. Both these researches were conceived by me quite independently.

My journey abroad was of great importance, above all, because it enabled me to make the acquaintance of such scientists as Heidenhain and Ludwig—men who devoted their whole life, with all its pleasures and sorrows, exclusively to science.

Until 1890, when I obtained a chair, I, now married and the father of a son, had always been hard up. However, thanks to the help of friends, as well as to my passion for physiology, I cannot say that this situation caused me any undue worry.

Finally, at the age of forty-one I was given a chair, my own laboratory, and filled two posts simultaneously; I was nominated professor of pharmocology (afterwards physiology) at the Military Medical Academy, and Head of the Physiological Department at the Institute of Experimental Medicine. Thus I suddenly found myself with ample financial means and every possibility for carrying out in my own laboratory any research work I liked. Before this the constant necessity to pay for every experimental animal, at a time when there was a scarcity of financial means, told heavily on laboratory work.

Since then life has gone quite smoothly, marked with ordinary laboratory and family events. The only thing which grieved me very much for a full decade was the strained atmosphere in the Military Medical Academy caused by its late chief.

In conclusion I must say that looking back on my life I would describe it as being happy and successful. I have received all that can be demanded of life: the complete realization of the principles with which I began life. I dreamed of finding happiness in intellectual work, in science—and I found it. I wanted to have a kind person as a companion in life and I found this companion in my wife Sara Vasilievna, née Karchevskaya, who patiently endured all the hardships of our existence before my professorship, always encouraged my scientific aspirations and who devoted herself to our family just as I devoted myself to the laboratory. I have renounced practicality in life with its cunning and not always irreproachable ways, and I see no reason for regretting this; on the contrary, precisely in this I find now certain consolation.

Above all I am forever grateful to my father and mother; they taught me to live a simple, unassuming life and made it possible for me to get a higher education.

I

PUBLIC AND SCIENTIFIC SPEECHES AND ADDRESSES

[MESSAGE FROM THE CHAIRMAN OF THE ORGANIZING COMMITTEE OF THE FIRST SECHENOV PHYSIOLOGICAL CONGRESS, READ AT THE OPENING OF THE CONGRESS ON APRIL 6, 1917[1]]

Dear Comrades,

I deeply regret that I am unable to be with you. We are living in really extraordinary times.

Hitherto dispersed and separated, we are coming together now and forming a society that will have common interests and a common aim—to maintain Russian physiology at the highest possible level. And our prime concern at the moment is our journal. One can say that in the permanent international exhibition of physiology we shall, at last, have our own pavilion! Each of us must do his best to make it as rich and as interesting as possible; it will give foreigners a better idea of our activity, better than they had when we were dispersed; it will enable them to appraise us. The circumstances favour the appearance of our journal. Our new intercourse in the form of regular papers from all parts of our motherland, exchange of views, demonstration of experiments and apparatus, and even, so to speak, entire physiological establishments—our laboratories—as well as the mutual encouragement and mutual assistance they entail—all this cannot but intensify our usual work. The present extraordinary situation in Russia is bound to add in a big way to our own particular upsurge.

We have just parted with the sombre epoch of oppression. It suffices to remind you that this congress was not per-

mitted to be held over Christmas, permission was granted for it to be held at Easter only after the members of the Organizing Committee had signed a statement saying that no political resolutions would be presented at the congress. But that was not all. Two or three days before our Revolution, final permission was obtained on the condition that the theses of the papers be submitted beforehand to the city head.

But thank God, this is already a thing of the past and, let us hope, the irrevocable past.

A grievous sin was committed by the Great French Revolution when it executed Lavoisier and when his appeal for a postponement that would enable him to complete important chemical experiments was answered by the statement that "the republic needs neither scientists nor their experiments."[2] But in this respect, too, the past century has seen a real revolution in the human mind; now we need not have any fears about a democracy disregarding the eternally majestic role of science in human life.

We cannot but anticipate, and with the new system of our life we must anticipate an exceptional growth of the means for all kinds of scientific activity.

And this being so, it should be an added incentive for us to step up our effort to the utmost.

How timely, then, in our free motherland, now being renovated and endeavouring to create the best possible conditions in all spheres of life, are the society and its journal, so happily linked with the glorious name of Ivan Mikhailovich Sechenov, the founder of Russian physiology and the embodiment of a genuinely free spirit!

Hearty greetings, comrades, and best wishes for a good beginning to our undertaking!

LETTER TO THE ACADEMY OF SCIENCES OF THE U.S.S.R.[3]

TO THE PRESIDIUM OF THE ACADEMY OF SCIENCES OF THE U.S.S.R.

I am most grateful to my own dear Academy for the greetings and good wishes. Whatever I do, I always think that I am thereby, as much as my strength allows me, serving first of all my country and our Russian science. And for me this is both a powerful incentive and a source of deep satisfaction.

Academician *Ivan Pavlov*

Leningrad, October 2, 1934

[LETTER TO THE SECHENOV PHYSIOLOGICAL SOCIETY, LENINGRAD[4]]

I express my sincere gratitude to the Sechenov Physiological Society for the celebration at a special session of my sixty years in science.

Yes, I am glad that, together with Ivan Mikhailovich, I and my group of dear colleagues have won for the mighty realm of physiological research, the animal organism, complete and undivided, instead of a vague half. And this, indisputably, is our Russian contribution to world science and generally to human thought.

Ivan Pavlov

Leningrad, October 14, 1934

[LETTER TO THE ACADEMY OF SCIENCES OF THE U.S.S.R.[5]]

TO THE ACADEMY OF SCIENCES

I am sincerely grateful for the warm comradely greetings. My dream is that our joint work in the laboratory shall leave its mark in the attainment of human happiness, and leave in the science that I love a memento worthy of the Russian mind!

Ivan Pavlov

Leningrad, December 23, [1934]

[A LETTER TO THE YOUTH[6]]

What would I wish for the young people of my motherland who dedicated themselves to science?

First of all—consistency. Of this very important condition for fruitful scientific work I cannot speak without emotion. Consistency, consistency and again consistency. Right from the very beginning inculcate in yourself the habit of strict consistency in acquiring knowledge.

Learn the ABC of science before you attempt to scale its peaks. Never embark on what comes after without having mastered what goes before. Never try to cover up the gaps in your knowledge, even by the boldest guesses and hypotheses. No matter how this bubble may delight the eye by its profusion of colours, it is bound to burst, and you will be left with nothing but confusion.

Develop in yourself restraint and patience. Never funk the hard jobs in science. Study, compare, accumulate facts.

No matter how perfect a bird's wing may be it could never make the bird air-borne without the support of the air. Facts are the air of the scientist. Without them you will never be able to take off, without them your "theories" will be barren.

But when studying, experimenting and observing, do your best to get beneath the skin of the facts. Do not become hoarders of the facts. Try to penetrate into the secrets of their origin. Search persistently for the laws governing them.

The second thing is modesty. Never think that you know everything. No matter in what high esteem you are held always have the courage to say to yourself: "I am ignorant."

Do not let pride take possession of you. It will result in you being obstinate when you should be conciliatory. It will lead you to reject useful advice and friendly help. It will deprive you of the ability to be objective.

In the team of which I am leader, everything depends on the atmosphere. All of us are harnessed to a common cause and each pulls his weight. With us it is often impossible to discern what is "mine" and what is "yours," but our common cause only gains thereby.

The third thing is—passion. Remember, science requires your whole life. And even if you had two lives to give they would not be enough. Science demands of man the utmost effort and supreme passion. Be passionate in your work and in your quests.

Our country is opening wide vistas before scientists, and—it must be owned—science in our country is being fostered with a generous hand.

What is there to say about the status of our young scientist? Here, it would seem, everything is quite clear. Much is given to him, much is expected from him. For him, as for us, it is a matter of honour to justify the great trust that our country puts in science.

I. P. Pavlov

[SPEECH AT THE OPENING OF THE FIFTEENTH INTERNATIONAL PHYSIOLOGICAL CONGRESS[7]]

I hereby declare the Fifteenth International Physiological Congress open. (*Applause and cheers. All rise.*)

On behalf of the entire Russian physiology I welcome the esteemed colleagues who have come from all parts of the world and hope that the time spent here will prove pleasant and useful to them.

For the first time an international congress of physiologists—the fifteenth—is being held in our country. This is in the order of things. Ours is a young physiology. Russia has only its second generation of physiologists, admittedly a hoary one that is living out its days. Sechenov must be regarded as the father of our physiology; he was the first to read lectures that were not lifted from the books of others, but as a specialist, illustrating them by demonstrations, and it was he who founded the first school of physiology in our country. All this, of course, was the result of his exceptional abilities. That is why we have considered it appropriate to present the members of this congress with his best works and with a medal bearing his image. Sechenov initiated physiological work on a considerable part of the earth's surface.

The manifold benefits of international congresses are so obvious and have been mentioned so often, that I shall merely draw attention to a few points of special significance at the moment.

It is high time that we, physiologists, as has been said here so many times, and as already practised at other congresses, reached a final decision on the so-called programme problems, i.e., on those problems which are the subject of particularly keen interest at the moment; at the same time, however, the papers on particular subjects should be read, too, although the number might be cut. General meetings should be arranged, and in addition to the disputants, those scientists working on the given problem invited beforehand. With such pre-arranged and stimulating conditions for discussion even off-hand remarks by colleagues not working on the given problem may assume no small importance.

The second point I would like to stress is one that is of particular significance for us, namely, the special influence of gatherings such as ours on the young generation of scientists, on the beginners. I know the effect of this influence from my own experience, from my early years in connection with the congresses of Russian naturalists and physicians of those times. Our government allocates extraordinarily large sums for scientific work, it is attracting masses of young people to science, and the spectacle of world scientific research represented here is bound to have a tremendous stimulating effect on this youth.

And finally, a third point. Although we are profoundly different, we are now united and stirred by keen interest in our common vital cause. We are good comrades and in many cases we are even linked together in manifest friendly feeling. We are working, obviously, for the rational and final unification of mankind. But should war break out many of us would become hostile to each other, and precisely on the grounds of our science, as has been the case more than once. In such circumstances we would have no desire to meet as we do now. Even our mutual scientific appraisal would radically change. I can appreciate the grandeur of a liberation war. At the same time, however, one cannot deny that war is essentially a bestial method of settling life's

difficulties (*loud applause*), a method unworthy of the human mind with its immeasurable resources. We observe now an almost world-wide desire and striving to avert war by means which are, perhaps, more reliable than hitherto. And I am glad that the government of my great country, in its fight for peace, has proclaimed for the first time in history: "Not one inch of foreign soil." (*Loud applause.*) And we, of course, must particularly sympathize with this struggle for peace and promote it. And as seekers of truth, we must add that it is necessary to be strictly just in international relations. (*Applause.*) But it is at this point we encounter the chief difficulty.

This year our truly world-wide association has lost two of its loyal members: Professor Shafer of Edinburgh University, who dedicated the whole of his long life to science, and Professor Macleod of the Aberdeen University, Nobel Prize winner, who died in his prime. Let us rise in honour of our late colleagues. (*All rise. The band plays Chopin's funeral march.*)

In conclusion, we, Russian physiologists, wish to express gratitude to our government which has enabled us to receive our esteemed guests in a worthy manner. (*Applause.*)

The chairman of the committee appointed by the government to further the work of the congress has the floor. (*Applause.*)

[SPEECH AT THE RECEPTION HELD BY THE GOVERNMENT FOR THE DELEGATES TO THE FIFTEENTH INTERNATIONAL PHYSIOLOGICAL CONGRESS ON AUGUST 17, 1935, IN THE GRAND KREMLIN PALACE]

You have heard of, and you have seen (says Ivan Petrovich addressing the foreign guests), the exceptionally favourable status enjoyed by science in my country. I would like to illustrate the relations that have been established in our country between the state and science by the following fact: we, the heads of scientific establishments, are really worried and alarmed because we are not sure whether we shall be able to justify all the allocations that the government has placed at our disposal. (Comrade Molotov interjecting: "We are sure that you will fully justify them." *Loud applause.*) As you know, I am an experimenter from top to toe. My whole life has been filled with experiments. Our government, too, is an experimenter but in an immeasurably higher category. I passionately want to live and to see the successful completion of this historic social experiment. (Pavlov's toast "the great social experimenters," is warmly applauded.)

[REPLY TO GREETINGS DURING A VISIT TO RYAZAN IN AUGUST 1935[8]]

I want to tell you that scientists were honoured in the past as well. But this expression of esteem took place inside a small circle, that is to say, among the same kind of people—scientists. That which I behold now has nothing in common with the restricted ceremonies of the past. At present science in our country is honoured by the whole people. I had evidence of this in the morning when I was met at the station, then in the collective farm and again on my way here. There is nothing fortuitous in this. I think I shall not be mistaken when I say that this is an achievement of the government of my country.

Formerly science was cut off from life, alienated from the people. Now I am seeing something altogether different—I see science honoured and prized by all the people. I raise my glass to the only government in the world that prizes science so highly and supports it so generously—to the government of my country.

[ON THE PROSPECTS OF WORK IN 1935[9]]

I am resting just now in my beloved Koltushi, and oh how I want to live for a long, long time. ... At least until the age of a hundred ... and even longer!...

I want to live for a long time because my laboratories are now in their heyday. The Soviet Government has allocated millions for my scientific work and for laboratory extension. I want to believe that the measures taken to encourage physiologists—and after all I remain a physiologist—will achieve their purpose, that my science will flourish on my native soil....

Whatever I do, I always think that in doing it I am, as much as my strength allows me, serving first of all my country. Social reconstruction on a grand scale is now under way in my country. The enormous gulf between rich and poor has been eliminated. I want to live and see the final results of this social reconstruction....

An enormous achievement of Soviet rule is the steady strengthening of the defence capacity of the country. One of the reasons why I want to live as long as possible is that I have no fears for the security of my native land.

[A MESSAGE TO THE GATHERING OF LEADING MINERS IN THE DONETS BASIN]

Dear Miners,

All my life I have loved and still love work, mental and physical, and the latter perhaps even more than the former. And I experienced the greatest satisfaction every time I succeeded in transplanting a good idea into my physical work, that is, when I was able to combine brain and hand.

You have taken the same path. My heartfelt wish is that you continue along this path, the only one that can ensure happiness for man.

With sincere greetings,

Academician *I. Pavlov*

January 7, 1936, Koltushi

II

WORKS ON BLOOD CIRCULATION AND THE TROPHIC ACTION OF THE NERVOUS SYSTEM

[AN ABSTRACT OF A PAPER BY V. N. VELIKY AND I. P. PAVLOV[10]]

V. N. Veliky and I. P. Pavlov have given an exposition of their joint works: a) *The Influence of the Laryngeal Nerves on Blood Circulation*, b) *The Centripetal Accelerators of the Heart-Beat.*

On the basis of experiments they have reached conclusions that are the opposite of those drawn by Schiff; they cannot, therefore, admit that there are in the laryngeal nerves of the dog fibres accelerating the heart-beat and emanating from the n. accessorius Willissi,[11] and have confirmed the experiments of Bezold and Cyon, which showed that the accelerating nerves come from the spinal cord through the ganglion stellatum.[12] This viewpoint is confirmed not only by their first work, but also by the second, which is not yet completed; it follows from the latter that there are centripetal accelerating nerves, which can be traced as follows: one of the nerve bundles proceeding from the heart enters the lower cervical ganglion in the angle formed by the n. laryngeus inferior and the n. vagus;[13] thence it turns into the ganglion stellatum and then, judging by one observation, it goes to the brain. The irritation of the central end of this nerve causes acceleration of the heart-beat. Consequently, it can be assumed that this is a sensory nerve and that its action on the heart is of a reflex character.

EXPERIMENTAL DATA CONCERNING THE ACCOMMODATING MECHANISM OF THE BLOOD VESSELS[14]

(PHYSIOLOGICAL LABORATORY OF PROFESSOR A. O. USTIMOVICH IN ST. PETERSBURG)

In a whole number of articles received in recent years from Professor Ludwig's* laboratory in Leipzig many of the outstanding properties of blood circulation have been touched upon and experimentally elucidated.[15] Thanks to these highly valuable experiments it has become known that 1) adaptability to larger or smaller amounts of blood is inherent in the vascular tube, the average blood pressure not showing any prolonged fluctuation over a considerable period; 2) this adaptability is of a nervous origin.

Nevertheless, it must be admitted that this opens up a new field for further investigation. No matter how significant the above-mentioned research may be, the elucidation of the mechanisms governing the adaptability of the blood vessels, as well as of their immediate properties, still remains a matter for future research.

These considerations have prompted us to carry out a number of investigations of the role of certain nerves in the accommodation of the blood vessels. But with the publication in the meantime of the results of various investiga-

* Collected Papers of the Leipzig Physiological Institute, 1873, 1874, 1875. (Papers by Tappeiner, Worm-Müller and Lesser.)

tions relating to the action of the vaso-dilatory nervous system, part of our own work provided for in our original plan of investigation, had to be dropped; still, this field of research proved extensive enough for our investigations as well.

Below we shall dwell only on one of a series of experiments the results of which will be published later. But we believe that this particular case, due to its promising significance, speaks for itself.

When numberless facts convinced us that the curve of the blood pressure in a curarized animal[16] cannot be always compared with the normal curve, and that in all cases without exception the given curve in an intoxicated animal undergoes certain fluctuations evoked by known and unknown causes, we deemed it necessary to carry out our first investigations on non-intoxicated, intact dogs. The dog selected for the purpose was tamed to such a degree that when the operation was being performed and the blood pressure taken, it lay perfectly still, tied to the operation board. Because of this we obtained curves of blood pressure which, in respect of evenness, can be regarded as model.

The manometer was as a rule connected with the artery which lies almost at the surface of the inside of the knee joint. Only two or three minutes are needed to bring the artery out and the operation is absolutely painless. The pressure in the art. cruralis was taken only once. The animal was well fed and given drink twenty-four hours before the experiment; twelve hours before the operation it was given drink once more.

After the blood pressure had been taken under these conditions, we began to feed the animal with dry bread or dried meat; the blood pressure was taken at different intervals after the feeding. It was established that the maximum decline of blood pressure (including that in the art. cruralis) reached only 10 mm. Hg.

Sometimes comparative measurements of the blood pressure within twenty-four hours after the feeding, recorded no changes whatever. It should be further pointed out that the pressure remained unchanged for 20-30 minutes after feeding and only then began to decline. These results once again confirmed the data obtained by Tappeiner, Worm-Müller and Lesser, according to which, in a normal state of the organism, too, there is, apparently, a tendency towards retention of average pressure. As is known, the above-mentioned states produce conditions that should have considerably contributed to a decline of blood pressure, namely, an appreciable dilation of the visceral arteries and secretion of large quantities of digestive juices from the blood flow; meanwhile the decline was a mere 10 mm. and sometimes the pressure even remained unchanged. Hence, the question arises: what kind of mechanisms maintain this equilibrium? Proceeding from the above-mentioned observations by Ludwig's pupils on the accommodation of the vascular tube, we considered it necessary first of all to find an answer to this question: is it really the case that the constriction of the blood vessels alone is responsible for this equilibrium of the blood pressure? Everything could have been accounted for simply by stating that simultaneously with dilation of the visceral vessels, there would be constriction of the vessels of other parts of the body, for example, of the skin, muscles, etc. It is possible that in these conditions there is a two-way reflex action of food, stimulating both the vaso-dilatory visceral nerves and the vaso-constrictory nerves of other regions. As is known, stimulation of the sensory cutaneous nerve evokes dilation of the cutaneous vessels and at the same time constriction of the visceral vessels. We had also to take into account the assumption that the reverse could have taken place, namely, that to stimulation of the sensory visceral nerves of the abdomen the abdominal vessels could have reacted with dilation, and the cutaneous vessels, on the contrary, with constriction.

We considered it important first of all experimentally to elucidate whether constriction of the cutaneous vessels is really caused by stimulation of the sensory visceral nerves.

We chose the ear of a rabbit as the most suitable object for our observations. Dissection of the internal organs served as a stimulus; we gave preference to this method over the application of electric and other stimuli because, above all, we wanted to reproduce as much as possible the conditions brought on by mechanical stimulation by food. In this respect electric stimuli seemed to us least suitable.

Our expectations were soon justified by the experiment. Each time the stretching of the intestinal loops out of the abdominal cavity of a curarized rabbit under artificial respiration caused constriction of the ear vessels, and this persisted for some time even after the peritoneum had been closed. Now it was necessary to preclude any suspicion of a passive reflux of blood from the ear vessels to the abdominal cavity, since it could be said that, given active hyperaemia of the abdominal vessels as a consequence of active dilation, the rabbit's ear revealed passive anaemia. Two kinds of experiments were carried out in order to refute this objection. In one case the cervical sympathicus was sectioned on one side and the effect of the dissection of the intestines on both ears, that is, on the intact and paralyzed vessels, was compared. In the other case, together with this comparative observation on the vascular lumen of both ears, the blood pressure in the art. carotis was measured.

Both series of experiments definitely proved that the constriction of vessels in the rabbit's ear under the dissection of the abdominal cavity is caused by a reflex transmission of the stimulation, since in the ear, on the side where the sympathicus was sectioned, no change whatever was observed in the lumen of the vessels, whereas in the ear vessel of the intact side, in all cases without exception, the dissection of the intestines led not only to the disappearance of the lumen, but also to the complete disappearance of the

vessel branch. Measurement of the blood pressure revealed the phenomenon already observed by Ludwig and Cyon, namely, no decline whatever; on the contrary increased blood pressure followed dissection of the peritoneum—a state which lasted from fifteen to sixty-six seconds after the abdominal cavity had been closed.

By way of illustration we give below a description of two typical cases taken from a series of experiments.

I. A curarized *rabbit*. The right art. carotis is connected with a mercurial manometer. The vessels of the left ear are under observation. (Chiefly it is the changes in the lumen of the medium branch of the artery that are taken into consideration.)

Time	The lumen of the vessel	Blood pressure
1 h. 07 m.	Of average width	
1 h. 08 m.	Narrower	
1 h. 09 m.	Still narrower	
1 h. 10 m. 1 h. 11 m.	Wider	
1 h. 12 m.	Narrower	
1 h. 13 m. 1 h. 14 m. 1 h. 15 m.	Still narrower	88
	The peritoneum is closed	
1 h. 15 m 30 s.	Considerably widened	106
1 h. 16 m. 30 s.	Narrower	95
1 h. 17 m. 1 h. 18 m. 1 h. 19 m. 1 h. 20 m.	Still narrower	92 90 87 85
	The peritoneum is opened **One intestinal loop is stretched**	
1 h. 20 m. 30 s.	Fully disappeared	115
1 h. 21 m.	Appeared again	89
1 h. 22 m. 1 h. 23 m.	Wider	86 Not measured
1 h. 24 m.	Narrower	85

Time	The lumen of the vessel	Blood pressure
	The peritoneum is re-opened	
1 h. 24 m. 30 s.	Practically disappeared	102
	The peritoneum is closed	
1 h. 25 m.	Wider	85
1 h. 26 m.		80
1 h. 27 m.	Narrower	77
1 h. 28 m.		76
	The peritoneum is opened	
1 h. 28 m. 30 s.	Disappeared	100
	The peritoneum is closed	
1 h. 29 m. 30 s.	Wider	87
1 h. 30 m.		74
	The peritoneum is opened	
1 h. 30 m. 30 s.	Disappeared	95
	The peritoneum is closed	
1 h. 31 m. 30 s.	Wider	77
1 h. 34 m.	Narrower	Not measured
1 h. 36 m.	Wider	74
	The peritoneum is opened	
1 h. 36 m. 30 s.	Disappeared	97
	The peritoneum is closed	
1 h. 37 m.	Appeared again	Not measured
	Artificial respiration interrupted	
1 h. 37 m. 30 s.	Considerably widened	Not measured
	The peritoneum is opened	
1 h. 38 m.	Disappeared	Not measured

A detailed analysis of the above figures would yield conclusions of no small importance. For example, one can hardly disregard the regular decline of the effect of stimulation of the intestines on the rise of the blood pressure, which represents, apparently, a phenomenon of fatigue. For the time being we shall abstain from such an analysis, especially since this case, as stated previously, must be regarded as only one of an extensive series of experiments.

In conclusion we should like to make one more casual observation.

When the abdominal cavity remained open for a longer time (one minute), or when opened more frequently for shorter periods, there was observed towards the end of the experiment, under interrupted respiration, a decline in the still considerable blood pressure (more than 60 mm. Hg), without any preliminary increase.

II. A curarized *rabbit*. The manometer is connected with the right art. carotis. The medium vessel of the left ear is under observation.

Time	The lumen of the vessel	Pressure in the art. carotis
4 h. 23 m.	Dilated	77
4 h. 24 m.		77
4 h. 25 m.		76
4 h. 26 m.		74
4 h. 27 m.		74
	The abdominal cavity is opened, one intestinal loop is stretched	
4 h. 28 m.	Constricted, almost disappeared	95
	The peritoneum is closed	
4 h. 29 m.	The medium artery disappears	75
4 h. 30 m.		74
4 h. 31 m.	Some branches of the vessel appear	70
4 h. 32 m.		69
4 h. 33 m.	The medium vessel appears	67
4 h. 34 m.	The vessels are filled to a still greater degree	64
4 h. 35 m.		Not measured
4 h. 36 m.	Constriction	60
4 h. 37 m.	Dilated	73
4 h. 38 m.	Continues to dilate	81
4 h. 39 m.	Considerably dilated	87
4 h. 40 m.		90

Time	The lumen of the vessel	Pressure in the art. carotis
	The peritoneum is opened	
4 h. 41 m.	Insignificant constriction	97
	The peritoneum is closed	
4 h. 42 m.	The lumen of the medium vessel is considerably constricted	88
4 h. 43 m.		83
4 h. 44 m.		79
4 h. 45 m.		76
4 h. 46 m.		75
4 h. 47 m.		76
4 h. 48 m.		79
4 h. 49 m.		76
4 h. 50 m.		75
4 h. 51 m.		75
4 h. 52 m.		77

In similar cases stimulation of the ischiadici,[17] even under a current of considerable strength, either did not produce any effect, or resulted in a decline of blood pressure instead of the usual increase. The ear vessels, on the contrary, remained unchanged. We leave the interpretation of this phenomenon until a detailed account of our experiments is published.*

* The above-mentioned observation is one of a series of experiments conducted in our laboratory by Mr. Pavlov in the autumn of 1876. Publication of the results of these experiments has been delayed for the reason that a number of similar papers on the same subject were published before Mr. Pavlov had completed his experiments. They will be published in the near future. (An editorial note of the Pfluger Archiv.)

CONCERNING TROPHIC INNERVATION[18]

It is perfectly clear that the horizon of medical observation of life is immeasurably wider than the sphere of vital phenomena which the physiologists have before their eyes in their laboratories. Hence the permanent incongruity between that which medicine knows, sees and empirically applies, and that which physiology can reproduce and explain. This relates, incidentally, also to the shock and neurotrophic phenomena of the clinic. For the former the physiologists have no generally accepted explanation; as for the latter, until now they cannot be observed under conditions of precise experimentation.

In the laboratory, however, not experimentally, but also clinically, I gradually came to the conclusion of the clinicians that there are special trophic nerves. Having operated for years on the digestive canals of animals (various fistulae, artificial separation and connection of different parts of the canal, etc.), for the purpose of facilitating experimentation during weeks, months and even years, I often unexpectedly observed extraneous and striking symptoms in surviving animals. I have read a number of papers on these symptoms at gatherings of the Russian Medical Society in St. Petersburg. I have seen various trophic disturbances of the skin and of the mucous membrane of the oral cavity as well as tetany and paresis; on one occasion I observed a case of acute and typically progressive paralysis of the spinal cord which lasted from ten to twelve days, on another occasion I observed a case of disease of the cerebral hemispheres (in

the form of strong infiltration) with complete distortion of the animal's normal attitude towards the external world; and finally I have seen shock phenomena, now quickly resulting in death, now manifesting themselves in temporary syncope of the animal almost fully simulating death. And all the cases were of a nervous character, either steadily progressive, or the reverse.

These observations gradually strengthened my supposition that such phenomena could be interpreted as reflexes coming from the abnormally stimulated centripetal nerves of the alimentary canal to the special inhibitory trophic nerves of various tissues. It has been assumed that the intensity of the chemical processes taking place in each tissue are regulated by special centrifugal nerves and in accordance with the principle inherent in the entire organism, i.e., in two opposite directions. Certain nerves intensify these processes and thereby increase the vitality of the tissue, while others weaken them, and, when subjected to excessive stimulation, deprive the tissue of its ability to resist the diverse destructive influences always acting inside and outside the organism.

On the basis of this assumption the observed shock phenomena were interpreted as being an acute, rapidly developing effect of an extremely strong reflex stimulation of the trophic inhibitory nerves, while the chronic pathological changes taking place in the tissues were seen as another effect of the same reflex stimulation, but weaker and more protracted.

In 1920 O. S. Rosenthal and myself deliberately operated on animals in a somewhat different way. Straining the nerves by means of displacing and fixing different parts of the alimentary canal, however, without any previous derangement of its integrity, we again observed many of the early symptoms, such as trophic diseases of the skin and of the mucous membrane of the oral cavity, paresis, and a considerable lowering of body temperature.

In this way we obtained additional proof for our contention that the phenomena observed by us are not conditioned by direct disturbance of the digestive process, as was the case in earlier experiments, when the animal was deprived of a more or less considerable quantity of digestive juices.

But, unfortunately, the pathological phenomena even now remained impermanent and fluctuating, and for this reason we were unable to go ahead with a strict and thorough analysis of their nervous mechanism. But these latest experiments strengthened our supposition, and at present we are trying out other methods in the hope of imparting greater stability to the phenomena which are of interest to us, especially since a thorough consideration of the subject brings together very many facts, both from the field of physiology and medicine, which lend weight to our supposition.

It may be that the trophic nerves, which to us are still hypothetical, have already been discovered by physiologists, and in the chief organ of the animal at that. Forty years ago physiology established the existence, along with the earlier known pair of rhythmic cardiac nerves—the retarding and accelerating nerves—another pair of cardiac nerves which might be described as influencing—also in an antagonistic way—the vitality of the cardiac muscle, i.e., raising it and lowering it. One of these nerves intensifies the heart-beat, conditions a more rapidly proceeding systole, increases the excitability of the muscle, eliminates the dissociation of the heart's sections and, generally speaking, heart troubles of all kinds whenever they emerge under unfavourable conditions. The influence exerted on the heart by the other nerve is the very opposite. What then, are these nerves? Perhaps they are the vascular nerves of the coronary system? But there are weighty experimental facts to disprove this: the action of these nerves is manifested on an excised, bloodless heart. And so one has to admit that they are trophic nerves.

Here is another case from the field of physiology. Long ago the late Heidenhain established two kinds of nerves for the salivary glands: one stimulating secretory activity of the glands in general, and the other accumulating in secretion their special organic substances. The first of these he named the secretory, the other the trophic nerve—with the reservation that the latter adjective was used conditionally, not in the generally accepted sense. Heidenhain's experiments, which later on were disputed in certain respects by some physiologists, have been definitely confirmed by the recent experiments of Professor B. P. Babkin. But is Heidenhain correct in regarding the term "trophic" conditional in his particular case? Indeed, no matter how liquid the saliva may be, it always contains all its components under stimulation of the secretory fibres. Consequently, the action of the trophic fibres must, undoubtedly, be interpreted as intensification of the constant vital chemism of the saliva, but then this relates to the function of the trophic nerves in the ordinary sense of this word. Just as the salivary glands have but one nerve, stimulating their function and having no antagonist, the trophic nerve, too, is a single nerve and acts positively.

Now for the medical aspect. I shall not dwell on special cases—these are well known to physicians and they are usually regarded as manifestations of neurotrophic disorders. I shall deal with the aetiology and therapy of some pathological cases, the mechanisms of which have not yet been disclosed by modern physiology.

Why do abnormalities in the digestive canal, especially of children, lead to various skin diseases? And on the contrary, why do certain influences on the skin cause diseases of the internal organs—of the pleura, lungs, kidneys, etc.? In the laboratory I observed in our dogs many cases of osteomalacia which often assumed a general and very acute character. Observation and even experimentation led me to conclude that this is caused by the chronic application of

damp cold to the skin, i.e., when subjected to cold the skin is continuously moistened.

Let us turn now to certain therapeutic methods. Why and how do compresses, mustard plasters, cupping glasses, etc., bring relief to the patient? Does physiology provide a satisfactory answer to this question? Obviously, there is a tremendous gap in modern physiology in this respect. But all the above-mentioned aetiological moments and therapeutic agents would become clear as to the mechanism of their action if we assumed the existence of an antagonistic pair of trophic nerves, now increasing the vitality of the tissue, now lowering it. They would then be cases of reflex stimulation of these nerves, at times causing illness due to a lowering of the tissue's vitality under strong, excessive stimulation of the retarding trophic nerves, and at others helping the tissue to overcome the morbific agents by increasing its vitality through stimulation of the positive trophic nerves.

Of course, this pair of nerves must constantly function also during normal working of the animal organism, but so far we, naturally, do not know when and how they are normally stimulated, the more so since we are not sure whether they exist or not. However, hypothetically we can visualize certain extreme cases of their physiological work under extraordinary conditions. Let us take for example the old and regular medical fact—a badly furred tongue due to indigestion. What does it mean and what mechanism is responsible for it? Indeed, we cannot always assume a continuous pathological process spreading from the stomach to the oral cavity. It may be assumed that the derangement of the stomach, and of the digestive canal in general, stimulates a reflex on the inhibitory trophic nerves of the mucous membrane of the mouth, and mostly of the tongue; this conditions a certain abnormal state leading to distortion and even loss of taste, since the receiving apparatus of the gustatory stimuli are located in the mucous membrane. But loss of taste causes abstention from food, and this gives the digestive canal a rest, which is a highly important thera-

peutic remedy against the pathological process. Thus, this would be a self-healing reflex on the part of the organism.

Let us take another instance. In cases of starvation the heart and brain, being the most important parts of the organism, preserve their normal weight longer than the other organs. It can be assumed that only in these organs is the normal energy of the vital chemical process maintained by corresponding reflexes on their positive trophic nerves, while in all other organs it is restricted and reduced, resulting in particularly rapid atrophy.

From a consistent point of view, the pair of trophic nerves must be the last, most direct distributor of the alimentary resources of the organism among its parts.

Thus, as we see it, each organ should be under ternary nervous control—the functional nerves initiating or inhibiting its functional activity (muscular contraction, glandular secretion, etc.); the vascular nerves regulating the bulk supply of chemical substances (as well as elimination of waste) by increasing or diminishing the supply of blood to the organ; and, finally, the trophic nerves which determine in the interests of the organism as a whole the exact quantity of material to be used ultimately by each organ. We have demonstrated this ternary control in the case of the heart.

Confining myself now to this insignificant factual report and continuing with the help of several colleagues laboratory investigation of our complex and difficult subject in different directions, I have taken the liberty of drawing your attention to this, mostly raw, working material of our laboratory research for a special reason. From the physiological aspect I wanted to spread among physicians the idea of trophic nerves, by introducing the concept of an antagonistic pair of these nerves and by stressing their probable universal and constant role in the organism. This, perhaps, will contribute to a more appropriate and, consequently, more fruitful analysis of the available clinical material.

III

WORKS ON DIGESTION

LECTURES ON THE WORK OF THE PRINCIPAL DIGESTIVE GLANDS

LECTURE ONE

GENERAL SURVEY OF THE SUBJECT. METHODS

Gentlemen,

The physiology of the digestive glands has engaged the attention of my laboratory, i.e., of myself and my co-workers, for many years, and we have obtained certain results which, it seems to me, are both of theoretical and practical importance. The secretory activity of the digestive canal, of its chief organs—the gastric glands and the pancreas—proved to be quite different from that usually described in text-books and, consequently, as pictured by the physician. We, therefore, considered it necessary to help in every way in establishing a revised and fuller teaching to replace the antiquated doctrines of the text-books. With this object I delivered a speech* at a meeting of the Society of Russian Physicians in St. Petersburg, dedicated to the memory of S. P. Botkin, outstanding Russian clinician. However, in the space of an hour I could only outline in general terms the results of years of work. It was impossible, in view of the shortness of time, to corroborate my words with documentary references, to convince my hearers by facts, by actual experiments. The lectures which I now submit to your es-

* Proceedings of the Society of Russian Physicians in St. Petersburg, 1894-95. (*Note By I. P. Pavlov.*)

teemed attention are designed to make good these deficiencies. The facts referred to in these lectures are taken from works which, for the most part, have already been published, but some unpublished facts obtained by our laboratory will also be adduced.

The digestive canal, in respect to its chief function in the organism, recalls a chemical factory where the raw material—food—undergoes a predominantly chemical treatment; this makes possible its absorption by the juices of the organism and its utilization by the organism for the maintenance of the vital process. This factory, then, consists of a series of departments in which the food, according to its properties, is more or less graded and then it is either retained for a time or immediately transmitted to the next department. The factory and each of its departments are supplied by special reagents produced, so to speak, in a primitive manner by the neighbouring small workshops situated in the walls of the factory itself, or by more distant and separate organs which may be compared with large chemical mills and which are connected with the factory by a system of tubes transmitting the reagents. These are the so-called glands with their ducts. Each factory delivers a special fluid, a particular reagent, possessing definite chemical properties; due to this, the reagent acts only on certain components of the food, usually a complex mixture of different substances. These properties of the reagents are chiefly determined by the presence of special substances in them, the so-called ferments. Definite reagents or digestive juices, as they are called, either act only on a single ingredient of the food, or on several, thus combining the inherent properties of many individual reagents, although each produces its own particular effect. But even a simple reagent, having only one ferment, represents a complex solution, since, in addition to the ferment, it may contain alkalies, acids, protein, etc.

All this has been studied by physiology through obtaining the above-mentioned reagents or pure ferments from the or-

ganism and investigating in test tubes their effects on the components of the food, as well as their interrelation. It is chiefly on this knowledge that the scientific theory of the processing of food in the organism, or as we say, of digestion, is based.

However, this theory of the digestive process, which is largely of a deductive character, obviously suffers from many serious defects. Without doubt there is still a considerable gap between the knowledge acquired, on the one hand, and the physiological reality, as well as the empirical rules of dietetics, on the other. Many problems still remain unsolved, or have not even been raised. Why are the reagents poured out on the raw material in one definite way and not in another? Why are the properties of separate reagents repeated and combined in other reagents? Are all the reagents always poured out into the digestive canal on any kind of food? Is each individual reagent subject to variation, and if so, when, how and why does it occur? Does the composition of all the reagents change simultaneously, or do individual reagents alter in different cases differently, depending on the kind of the raw material? What happens to the reagents when the activity of the entire factory increases or diminishes? Is there not something in the nature of a contest between definite components of the food, i.e., does it not happen that some of them require a special reagent which may interfere with the successful action of other reagents on the remaining components?, and so on, and so forth. No one, of course, can doubt that these questions are relevant to the case. The mechanism of the digestive process cannot be presented in the abstract manner typical of present-day physiology. The individual properties and the diversity of the reagents clearly indicate that the work of the digestive canal is highly complex, delicate and strictly adapted to the given digestive function. Upon reflection, we must admit a priori that for any food, i.e., for any combination of substances which are to be processed, there is a definite combination of reagents with their special proper-

ties. No wonder that dietetics is, if not in its general empirical principles, then in its explanations and particularities, one of the most intricate branches of therapy. The physiologist must have knowledge not only of the elements of digestion—of the effects of individual reagents; in order fully to master the subject he must also include in the sphere of his observation the entire actual process of digestion. This, of course, was realized by many investigators who attempted to accomplish it, and they would have succeeded had the knowledge been more easily attainable.

Full knowledge of the digestive process can be acquired in one of two ways—on the one hand, by investigating the state of elaboration of the raw material in each part of the digestive canal (the method of Brücke, Ludwig's[19] school, etc.) and on the other hand, by strictly ascertaining when and what quantity of the reagent is poured out into the digestive canal on each kind of food, as well as on the entire meal, and what its properties are (this way was taken by numerous researchers who investigated the secretory activity of the digestive glands).

Our investigations belong to the second category. The earlier investigations were hindered by the inadequate methods employed. It is often said, and not without reason, that science advances in leaps, depending on the development of experimental methods. With every advance in method, we rise, so to speak, a step higher, and a wider horizon with hitherto imperceptible objects unfolds before us. Our first aim, therefore, was to develop a method. We had to observe how the reagents were poured out on the food coming into the digestive factory. Ideal accomplishment of this task required the fulfilment of many and difficult conditions. It was necessary to have the reagents at hand *at any time*, otherwise important things might escape us; the reagents had to be obtained *in an absolutely pure condition*, otherwise we would not have been able to establish the variations taking place in their composition; we had to *ascertain their quantities* accurately, and, finally, it was

necessary that *the digestive canal should function normally*, and that the animal *should be in good health.*

It is understandable that physiology approached the solution of this problem gradually, much effort having been spent in vain, and a number of attempts having been unsuccessful, although many outstanding scientists devoted attention to this problem.

We shall begin our consideration with the pancreas—a fairly simple matter. It might seem that here our task is quite easy. We need only find the duct through which the gland secretion is transmitted to the digestive canal, and then, by attaching a cannula to it, let the fluid flow to the outside, into a graduated vessel. This, actually, is not such a difficult matter, but, unfortunately, it does not solve the problem. Although the digestive process in the animal under experiment is in full swing, after this operation, in most cases, there is no flow of pancreatic juice at all, or if there is any its quantity is abnormally small. In this case observations on the process of secretion, as well as on alterations in the composition of the juice caused by different kinds of food, are out of the question. Further investigation showed that the pancreas is a very delicate organ, and as a result of the conditions inevitably accompanying the operation (narcotization, dissection of the abdominal cavity, etc.) its state of disturbance is such that in most cases no traces of normal activity remain. This method is known in science as the temporary pancreatic fistula. Its failure, naturally, resulted in attempts being made to find new methods.

A possible way out was to obtain the juice from the duct not during the operation period but after it, when the inhibitory influence of the operation had fully vanished. For this it was necessary to let the juice escape from the duct for a considerable length of time. It was believed that this could be done either by attaching a glass tube to the animal's duct and bringing it out to the exterior through the abdominal wall (Claude Bernard),[20] or by placing in the

duct a T-shaped piece of twisted lead wire (Ludwig's school). These methods were called permanent pancreatic fistula. They proved effective, but only for a short time, usually for three to five days and in exceptional cases up to nine days. After this the glass tube usually fell out and the fistula closed up; the lead wire, too, failed to prevent this. And so these methods too had to be regarded as bearing a temporary character. But this was not their only defect. In two or three days when the inhibitory influence of the operation had passed away, there was manifested in many cases another abnormal state—a continuous excitation of the gland irrespective of the fact whether the dog was fed or not. This prompted the question, which of the two was better—the temporary or the permanent fistula? But it was obvious that both were defective. Whereas with a temporary fistula the normal conditions were almost invariably distorted due to the inhibitory influence of the operation, with the so-called permanent fistula there was often observed an inflammatory process in the pancreas which manifested itself a few days after the operation (especially in those performed in the old laboratories) and which also distorted the normal activity of the gland.

It only remained to find a means of access to the gland lumen which would keep the duct open for any length of time, until the above-mentioned unfavourable conditions had disappeared completely. Such a means was first suggested by me in 1879 and a year later, in 1880, by Heidenhain independently.[21]*

My method was this (I shall describe the method of my operation which differs slightly from that of Heidenhain): from the wall of the duodenum a diamond-shaped piece, containing the natural opening of the pancreatic duct, is cut out; the intestine, the lumen of which is not appreciably narrowed, is stitched up, and the separated piece of intestine is sewn (with the mucous membrane outwards) into

* Hermann's *Handbuch der Physiologie*, Bd. V.

the opening in the abdominal wall. The wounds heal quickly, and the entire operation requires no special skill; it does not last long (about half an hour) and is easily endured by the animal. After two weeks the animal is absolutely ready for observation. In the place of the healed-up abdominal wound there appears a roundish elevation of the mucous membrane, 7 to 10 mm. in diameter, with a cleft-like orifice which in the more successful cases is located exactly in the centre of the elevation. If the animal is now fastened in the stand, the juice can be collected either directly, as it drops from the mucous papilla, or if it flows along the abdominal wall, by properly fixing a funnel, with the wide end upward, to the abdomen. The two obstacles, which hindered the work of the investigators when the temporary and so-called permanent fistulae were employed, have been removed. The gland, undoubtedly, is now in a normal condition; however, the trials of the experimenter are by no means over.

In a short time the abdominal wall, owing to the action of the escaping juice, becomes greatly eroded and large areas of it even begin to bleed. This irritates the animal and interferes with the collection of pure juice by means of the funnel. What is to be done? Many things may help—frequent washing of the affected skin with water and application of emollient ointments; even better results can be obtained if the animal is kept fastened in the stand for a number of hours every day, with the funnel attached to its abdomen. But best of all is to let the animal, when free from experimental work, to lie on a bed of porous material, such as sawdust, sand, or old mortar. Many animals find the most suitable position in which to lie, namely, on the abdomen, so that the escaping juice is immediately absorbed by the porous material and the overflow of juice and skin abrasion is fully and readily avoided. It is worth mentioning that this method was adopted as a result of a hint given by one of the dogs subjected to the operation.

I shall take the liberty of telling you about this interesting case in detail. In one of the dogs operated according to our method the eroding action of the juice began to manifest itself ten to fifteen days after the operation. The dog was tied in the laboratory. One morning, much to our annoyance, a heap of plaster torn from the wall was found beside the animal which was generally known for its quiet behaviour. The dog was then chained in another part of the room. Next morning we observed the same thing—another part of the wall had been damaged. At the same time it was noticed that the dog's abdomen was dry and that the cutaneous irritation considerably diminished. Only then did we realize what had caused the dog's strange behaviour. When we prepared a bed of sand for the dog, the wall was no longer damaged and the flow of juice ceased to trouble the animal. We (Dr. Kuvshinsky and I) gratefully acknowledged that by its manifestation of common sense the dog had helped us as well as itself. It would be a pity if this fact were lost for the psychology of the animal world. And so another obstacle had been overcome, but the final goal had still to be reached.

Three or four weeks after the operation, the animals, which previously seemed in a normal state, suddenly became ill; they began to reject the food and showed signs of rapidly developing weakness, which in most cases was accompanied by convulsive symptoms and sometimes even by violent convulsions, followed, after two or three days, by death. Obviously, this was a peculiar form of disease. To think in terms of inanition was out of the question, since animals often die with their weight almost at the normal level; the supposition of some form of post-operative disease, such as chronic peritonitis, was also excluded, since neither the state of the animals before death nor the findings of the autopsy justified this. Finally, we had to give up the idea of the possibility of any self-intoxication caused by food insufficiently or incorrectly digested as a consequence of the digestive canal losing a considerable quan-

tity of pancreatic juice—an idea suggested by Dr. Agrikoliansky* in his dissertation. In the first place, no symptoms of digestive disorder were observed in many animals before death—neither vomiting, nor diarrhoea, nor constipation. In the second place, our experiments, in which the pancreatic duct was specially ligatured and sectioned, had demonstrated the absolute harmlessness of this operation. There remained but one assumption, namely, that together with the escaping pancreatic juice the animal had lost something that was essential to the proper course of the vital processes. Proceeding from this idea we applied two means of protecting our animals against possible complications. Aware of the powerful influence exerted by different kinds of food on the composition and quantity of the secreted pancreatic juice, we (Dr. Vasiliev) excluded meat from the diet of the dogs, feeding them solely on bread and milk. On the other hand, taking into account that with the escaping pancreatic juice the organism loses a large quantity of alkali, we regularly added a certain quantity of sodium bicarbonate to the food (Dr. Yablonsky).

With the help of these two measures it is quite easy to obtain an animal provided with a permanent pancreatic fistula and at the same time fit for experimentation for a period of months and even years, without additional precautionary measures. Of course, the difficulties encountered in handling different animals greatly vary. As a rule one of every four or five dogs endures the operation without any special subsequent care. The way in which the sodium bicarbonate helps is not yet clear. It is possible that the sodium really compensates for the injurious deficiency of alkali in the blood; however, it is likewise possible that its action consists, as shown by Dr. Becker, in reducing the secretion of the juice. In the latter case the nature of the substance the loss of which proves so inju-

* "The Influence of Strychnine Nitrate on the Secretion of Pancreatic Juice in the Dog," Dissertation, St. Petersburg, 1893.

rious to the organism, would remain obscure. Quite clearly this question is of great importance, since here we have a new, experimentally induced, pathological state of the organism. In our laboratory, investigation of this problem has been undertaken by Dr. Yablonsky.

The collection of the juice is performed by means of a glass, or, better still, of a metallic funnel, with its wide end upwards and pressed to the spot containing the opening of the pancreatic duct with the help of elastic bands or simply rubber tubes tied round the body. On the funnel there are hooks to which small graduated cylinders are attached; the animal is placed in the experimental stand. While these arrangements are very convenient for the observer, the animal is not at all comfortable, especially when the experiment lasts a long time; it becomes tired and restless. However, it gradually learns to sleep soundly even in these conditions, especially when its position in the stand is eased, for example, by supporting its head. In cases when dogs are first used for laboratory work it is better to collect the juice in a lying posture, placing a vessel under the opening of the duct and slightly pressing it to the body.

I have deliberately described the series of misfortunes experienced by us in connection with the formation of a permanent pancreatic fistula; I wanted to show how difficult it is to solve apparently easy problems when dealing with material such as ours.

There is no doubt that our solution of the problem is also far from being ideal. What is badly needed is a method that would allow the juice to flow outside during the experiment and into the intestine during the intervals. In addition to saving much juice for the organism, this method would be of particular importance because it would preclude the possibility of any considerable changes in the work of the digestive glands in general. There are certain grounds for assuming that the steady flow from the digestive canal of such important a reagent as the pancreatic juice is com-

pensated for, to a degree, on the one hand, by a heightened or otherwise changed activity of the remaining digestive glands, and, on the other hand, by the expedient depreciation of the juice which is uselessly and continuously poured out on the floor. But the significance of these somewhat far-fetched hypotheses must not be overrated. Later we shall see how clear, unquestionable and instructive are the results of the investigations carried out with the help of this method. The method recently published by the Italian scientist Fodera* approximates, in a way, to the perfect, irreproachable method. He succeeded in placing in the duct a T-shaped metallic cannula which, apparently, makes it possible to collect the juice on the outside, or to direct it to the intestine by closing the outer end of the cannula. This method, however, suffers from one essential defect: there is no guarantee that simultaneously with the flow of juice to the outside a certain quantity of it may not enter the intestine.

No less difficult and protracted was the evolution of methods for obtaining the gastric juice and for observing its secretion. Leaving aside the older and obviously inadequate methods, we shall dwell in detail on the formation of a gastric fistula, as the starting-point for the method employed at present. In 1842, our countryman, Professor Basov** and in 1843, the French physician Blondlot*** independently, suggested the idea of artificially reproducing in animals the condition observed by an American physician in one of his patients; the latter had a permanent unhealable opening in the abdominal wall leading to the stomach —it had been caused by a bullet wound.[22] Both Basov and Blondlot, using dogs, made an opening through the abdominal wall into the stomach and fastened into it a metallic tube corked from the outside. The tube heals into the

* *Moleschotts Untersuchung zur Naturlehre des Menschen und der Tiere*, Bd. XVI, 1896.

** *Bulletin de la Soc. des natur. de Moscou*, t. XVI.

*** *Traité analytique de la digestion*, 1843.

wound and can remain in this position for years without causing even the slightest harm to the animal.

This method raised great hopes at the time, since it provided easy and free access to the inside of the stomach at any moment. But as time went on these hopes gave way to an ever-increasing disappointment. For the purpose of investigating the properties of the ferment of the gastric juice almost all researchers had to use extracts prepared from the mucous membrane of the stomach, since only very little and highly impure juice could be obtained through the fistula. It was also very difficult to observe the course of gastric secretion during digestion and to obtain an idea of the properties of the gastric juice in different conditions, since the juice was mixed up with the mass of food. The result was that voices began to be raised saying that the gastric fistula had justified none of the hopes, that it was hardly of any use at all. However, this was an exaggeration due, apparently, to the disappointment at the slow progress in elaborating the theory of the secretory activity of the digestive canal, and of the gastric glands in particular. Indeed, many important observations had been made earlier with the help of the gastric fistula. Now it was only necessary to introduce a slight modification in it in order, with its help, to make possible the final solution of a number of fundamental problems.

In 1889, we (Mrs. Shumova-Simanovskaya and I) performed the oesophagotomy operation on a dog with an ordinary gastric fistula: we severed the oesophagus at the dog's neck and sutured both its ends separately to the edges of the skin wound. We accomplished thereby the complete anatomical separation of the mouth and stomach cavities. Animals subjected to this operation fully recover if well cared for, and live for many years in perfect health. In feeding, the food, naturally, is introduced direct into the stomach. The following interesting experiment can be performed with such animals. The dog is given meat, which, of course, falls out through the upper opening of the

oesophagus. However, in the perfectly empty stomach, preliminarily washed out with water, there begins a profuse secretion of absolutely pure gastric juice, which continues all the time the animal is eating, and even for some time longer. Hundreds of cubic centimetres of gastric juice can be easily obtained in this way. I shall discuss in future lectures the causes responsible for the secretion of gastric juice under such conditions, as well as the significance of this phenomenon for the entire process of digestion. Now I shall merely remark that the problem of obtaining pure gastric juice has been definitely solved by means of this method; at present it is possible to collect from an animal thus operated upon a few hundred cubic centimetres of gastric juice every two days or even every day, without causing any apparent harm to its health, i.e., to obtain gastric juice from a dog almost as regularly as one obtains milk from a cow.

For our ferment experiments we no longer need to prepare an infusion of the mucous membrane; we now obtain from the living animal enormous quantities of the purest ferment with much greater ease and in less time. The animal subjected to the operation becomes an inexhaustible source of the most refined product. It seems to me that the pharmaceutist, too, should devote attention to this fact, since the physician has always regarded pepsin and hydrochloric acid as being beneficial and, in many cases, essential. Comparative experiments carried out in detail by Dr. Konovalov with solutions of commercial pepsin and natural gastric juice obtained from dogs as described above, showed that the former could by no means compete with the latter. The possible objection that the gastric juice is obtained from dogs can hardly be regarded as a serious obstacle to its widespread use as a pharmaceutical preparation. In the laboratory we tried it out on ourselves, and the tests revealed beneficial rather than injurious effects. The taste is by no means unpleasant and it contains nothing extra compared with a corresponding solution of hy-

drochloric acid. In view of the prejudice, it is quite possible to obtain gastric juice in a similar way from other animals whose flesh is consumed by man. I cannot but express regret that this method which, in any case, deserves a serious trial, is not promoted in Russia, although I have frequently called the attention of my medical colleagues to it. The desire to try my luck once more has made me dwell on this collateral subject when describing our methods. Since last year pure gastric juice, obtained by Dr. Fremont from the isolated stomach of the dog by the method of Thiry's well-known intestinal fistula, has been recommended abroad as a therapeutic remedy for various affections of the digestive canal. Perhaps this product, actually long known to us, would have more success in our country if it appeared under a foreign flag!

But to return to the subject of our methods. As already mentioned, the problem of obtaining pure gastric juice has been solved, but as yet no progress has been made in providing the means which would make it possible to observe the secretion of the juice and to study its properties during digestion.

Obviously this requires adherence to an absolutely exceptional condition—normal gastric digestion together with strict collection of perfectly pure juice. That which is quite simple in the case of the anatomical relations of the pancreas (where the cavity containing the food is fully separated from the cavity containing the juice), becomes a matter of the greatest difficulty in the case of the stomach, for its glands are microscopic and are situated in the walls of the cavity containing the food. A truly fortunate idea for overcoming these difficulties was suggested by Thiry. In order to procure pure intestinal juice, which is also produced by microscopic glands situated in the intestinal wall, and to be able to study the course of its secretion, Thiry cut out a cylindrical piece of intestine, shaped it into a cul-de-sac and sewed it into the opening of the abdominal

wound. This idea was used by Klemensiewicz* in 1875 for obtaining pure juice from the pyloric end of the stomach, but his dog died three days after the operation. Heidenhain,** however, succeeded in nursing such a dog and keeping it alive. Shortly afterwards Heidenhain*** isolated a piece of the stomach fundus and gave it the shape of a cul-de-sac, which secreted its juice externally.

Thus the above-mentioned requirement was met. When, in the normal way food reached the large stomach, which remained in its usual position, the isolated pouch began to secrete perfectly pure juice, the quantity of which could be accurately measured at any interval of time. However, in order to arrive at well-founded conclusions concerning the normal work of the stomach during normal digestion, judging by the activity of the isolated pouch, it was necessary to ensure its absolute nervous inviolability. Evidently in Heidenhain's operation this was not the case, since in making the transverse incisions by which the piece of stomach was cut out, the branches of the vagus extending lengthwise along the wall of the stomach were severed. Consequently, before the method could be improved this defect had to be eliminated.

In order to do this, we (Dr. Khizhin and I) modified Heidenhain's operation in the following way. The first incision, which begins on the side of the fundus two centimetres from the pars pyloris, is carried in a longitudinal direction for 10 or 12 centimetres through the posterior and anterior walls. In this way a triangular flap is formed. A second incision is made precisely at the base of this flap, but only through the mucous membrane, the muscular and serous coats being left intact. The edges of the incised mucous membrane are separated for one to one and a half centimetres from the subjacent tissue on the side of the stomach, and on the side of the flap for two to two and a half centimetres. The edge be-

* *Sitzungsbericht der Wiener Akademie*, 1875.

** Pfluger's Archiv f. d. ges. Physiologie, Bd. XVIII. 1878.

*** *Ibid.*, Bd. XIX, 1879.

longing to the large stomach is folded and stitched together. The edge of the flap is shaped into a cupola. The stitching along the edges of the first incision both in the stomach and the flap forms a septum between their respective cavities consisting of two layers of mucous membrane; one of these layers is intact, the other being sutured along the middle. Only thanks to the above-mentioned cupola it is possible to obtain an experimental animal with a permanent fistula; if both layers of the mucous membrane were sewn along the middle, then after a shorter or longer period of time a communication would be formed between the stomach and the cul-de-sac, and this would make the animal unfit for our purpose. Better still would be to form cupolas out of the mucous membrane on both sides of the stomach. To make a long story short, we cut out an elongated piece from the stomach, shape it into a cylinder, suturing its free end into the opening of the abdominal wound and allowing the other end to remain connected with the other part of the stomach; the stomach becomes separated from the cul-de-sac by a septum of the mucous membrane. For the sake of illustration I give here schemes of the operation, taken from the work by Dr. Khizhin (Fig. 1).

Naturally our addition to Heidenhain's operation makes it much more complicated, but, as we shall see later from experiments, this difficulty is compensated for by the absolutely intact condition of the nervous relations of our artificial stomach; this is clear from the fact that the fibres of the n. vagi pass between the serous and muscular layers of the bridge into the isolated pouch. This operation does not cause any serious discomfort to the animal and does not endanger its life.

It would be appropriate now to answer the question whether the activity of our miniature stomach provides a true reproduction of the secretory work of the large stomach, since in the latter the food comes into contact with the walls during the normal process of digestion, while the former remains empty. However, I shall answer this

question fully in a later lecture, when we have in our possession more facts for the solution of the problem. Now I shall merely state that, in addition to strict conclusions drawn from a series of unquestionable facts, we carried out *numerous experiments in which the miniature and large stomachs were directly compared* as to the conditions of

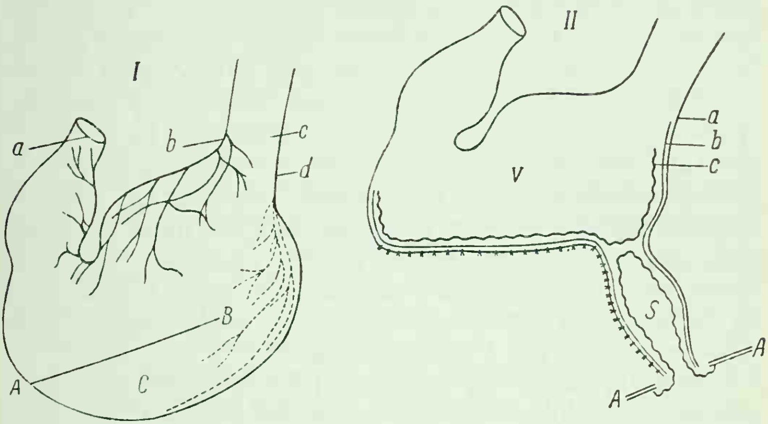

Fig. 1. I: *a*—Pylorus; *b*—Plexus gastricus anterior vagi; *c*—Oesophagus; *d*—Plexus gastricus posterior vagi; *AB*—Line of incision; *C*—Flap for forming stomach pouch; II: *a*—Serosa; *b*—Muscularis; *c*—Mucosa; *A*—Anterior abdominal wall; *S*—Cavity of stomach pouch; *V*—Cavity of stomach.

work and properties of secretions; as a result no room was left for doubt that the miniature stomach, *on which* we base our study of normal gastric activity, is fully valid for our investigations. In my next lecture the miniature stomach will appear as an instructive object worthy of close attention.

As mentioned previously, Dr. Fremont succeeded recently (after publication of our method) in isolating the whole stomach of a dog in accordance with Thiry's principle; he connected the lower end of the oesophagus with the duodenum, and fixed an ordinary fistular cannula in the

stomach closed at both ends. This method, however, can only serve for certain special experiments on gastric secretion, as I shall show later on. As a general method it suffers from two essential defects. First, in ordinary digestion in such dogs it is impossible to reckon on absolutely normal conditions of gastric secretion, since the mucous membrane of the stomach is not in the slightest degree reflexly excited by contact with food; secondly, when food is introduced directly into the stomach it mixes with the gastric juice. As to obtaining juice for practical purposes from such a stomach, it seems to us that our method of combining an ordinary gastric fistula with oesophagotomy possesses greater advantages compared with Dr. Fremont's method. Our method is incomparably simpler from the surgical point of view, and, given proper conditions for the operation, does not result in useless sacrifice of animal; the animals subjected to the operation live for years, enjoying perfect health. Can this be said of Dr. Fremont's dogs?

The usual method of collecting juice from our miniature stomach is as follows: a glass, or better still, a rubber tube is introduced with its perforated end into the cul-de-sac. This tube either remains there of itself, or it is secured by means of an elastic band round the animal's body. The juice is collected either in the lying or standing posture.

It seems to me now that the method of forming an isolated miniature stomach must be regarded as the only possible one and fully correct in principle. There still remain some minor defects, such as the erosion of the edges of the wound and the loss of a certain quantity of gastric juice, but they can be easily eliminated or regarded as being of little importance; in time they may be altogether avoided.

In the interests of a better study of the entire secretory activity of the digestive canal simplification of the technical side of the described methods is to be desired; it is necessary to remove the minor defects so that it may be

possible to make several fistulae on one and the same animal without causing any danger to its life or health.

It is clear from the foregoing general survey of the digestive process that the study of the concordant work of the separate glands is of great importance; but it can be carried out with absolute exactitude as regards time, intensity, etc., only when the activity of all or many glands is simultaneously observed on one and the same animal.

In concluding this description of methods I consider it essential to dwell for a moment on the importance of surgical technique in physiology. It seems to me that in modern physiology a firmer stand is to be taken by the surgical method (I counterpoise it to the purely vivisectional method), which implies performing (skilfully and creatively) more or less complex operations with the aim of either extirpating certain organs, or of ensuring access to physiological phenomena taking place in the depths of the body; of disrupting one or another connection linking the organs, or, on the contrary, establishing a new connection, and so on; it also implies ability subsequently to heal the animal and restore its state, in the measure that the nature of the operation allows, to normalcy.

I regard the promotion of such surgical technique to be a matter of the greatest importance, because the usual method of simply vivisecting the animal in an acute experiment is, as is now becoming clearer day by day, a major source of errors, since the act of crude violation of the organism is accompanied by a mass of inhibitory influences on the functions of the different organs. The organism as a whole, the realization of the most delicate and most expedient linking of an enormous number of separate parts, cannot, in the nature of things, remain passive to destructive agents; it must, in its own interests, strengthen one part and weaken another, i.e., temporarily leaving aside, so to speak, all other aims, and concentrating on saving whatever can be saved. While this circumstance has been and still is a big obstacle in the way of analytical physiol-

ogy, it appears to be an insurmountable obstacle to the development of synthetic physiology where it is necessary to determine exactly the true course of one or another physiological phenomenon in an intact and normal organism. At the same time discovery in the sphere of performing operations, as a means of physiological research, has in no way disappeared; on the contrary, as life shows, it is just coming into its own. Recall, for example, the extirpation of the pancreas performed by Minkovsky;[23] the transference of the portal blood into the vena cava by Dr. Eck;[24] and, finally, the remarkable operations of Goltz in which he extirpated different parts of the central nervous system. Have not many physiological problems been solved in this way, and do they not give rise to numerous new problems? It may be argued that such operations have been performed already. Yes, but, in the first place, they are very rare and performed only by the few. If, for instance, the number of physical instruments devised annually and introduced for the investigation of physiological phenomena, as well as the number of physiologico-chemical methods and their variations, be compared with the number of new physiological operations after which the animal must survive, the meagreness of the latter stands out in marked contrast to the richness of the former. In the second place, it is noteworthy that many of these operations were performed first by surgeons and not by physiologists; the latter, so to speak, do not regard these problems as being essential for their work, or do not possess the means necessary for their solution. Finally, the fact that surgical methods have not yet taken their proper place in physiology is demonstrated in the most striking fashion by the absence in modern physiological laboratories of properly equipped surgical departments, whereas provision is made for chemical, physical, microscopic and vivisectional departments.

The ordinary laboratory rooms cannot be used for frequent and complicated operations ensuring the survival of the animals, without the sacrifice of much time and labour; such

operations require all the methods and conditions provided by present-day surgery. There is no doubt that certain operations, even performed with the aid of antiseptic and aseptic precautions, would not be successful in an ordinary laboratory, since, when dealing with animals, it is almost impossible to maintain absolute cleanliness during and immediately after the operation in the absence of a large surgical department specially equipped for this purpose. I shall refer by way of example to the well-known history of Eck's fistula which connects the vena cava inferior and the portal vein. In the conditions that prevailed in the old laboratories the inventor of this operation, despite his energy and resourcefulness, could not keep the animals alive for any considerable period after the operation. Similar failure overtook Prof. Stolnikov, who repeated the operation with the assistance of Dr. Eck sparing neither dogs nor effort. It was only in the surgical department of the physiological laboratory of the Institute of Experimental Medicine, which had just been opened (1891), and consequently, in a building which, in the surgical sense, was perfectly clean, that the first major success was registered. However, this happy period of successful operations lasted for only one year. The physiological laboratory in the Institute was a small place at that time, and therefore, despite precautionary measures, it quickly became so impure that the Eck operation, though performed by the same, now even more skilful hands, proved fruitless and a waste of time. This situation lasted for about a year, notwithstanding all the efforts made by the experimenters, until a new physiological laboratory was built in the Institute where considerable space was allotted to the surgical department.

I take the liberty of calling your attention to this, because, as far as I know, it is the first instance of a special surgical department in a physiological laboratory. Perhaps this example will give my physiological colleagues some useful hints for designing new institutes. The surgical department occupies half the upper floor which comprises a

quarter of the entire laboratory. It consists of a set of operating rooms located along one side; in the first room the animal is washed in a bath and dried on special stands; in the next (the preparation-room) the animal is narcotized, and the site of the operation shaved and cleansed with antiseptic solutions; the third room is used for sterilizing the instruments and cloths, for washing hands and donning overalls; the fourth is a well-lighted operating room. The narcotized and prepared animal is carried, without any table, into this room by those who take part in the operation. As a rule the attendants are not allowed to go beyond the second room of the operating section. This set of rooms is separated by a solid wall from a series of special cabins where the dogs are kept for the first ten days after the operation. Each of these cabins has a large window with a small hinged pane for ventilation; its floor space is about one square sazhen,* and it is more than five arshins** high. Each cabin is heated with hot air and provided with electric light. There is a passage running along the cabins, each being shut off from it by a massive, tightly-fitting door. The entire department has cement floors with gutters in each room. In the cabins a lead pipe with small apertures in it runs along the walls near the floor, and by means of this pipe the floors can be watered from the corridor at any time without entering. The whole department is painted a white oil colour. The long series of operating rooms is a reliable protection against penetration of dirt into the last and main operating room. Although physiology owes much to the intelligence of the dog in general, it would be in vain to rely on the assistance of this clever animal in carrying out certain surgical tasks. Only by arranging this long series of barriers against penetration of dirt, was it possible, in the ordinary and surgical sense of the word, to maintain the surgical department in the proper condition for a considerable length of time. Two years

* An old Russian measure of about seven English feet.—*Tr.*

** An old Russian measure of about twelve English feet.—*Tr.*

of work in this department have not made it impure, which is proved by our criterion for surgical cleanliness—the successful performance of the Eck operation. When I recall the results of operations carried out in the course of the last twenty years in different buildings, and always upon equally healthy material, with a frequent repetition of the same operation, I am amazed, perhaps even to a greater degree than the surgeon, at this triumph of cleanliness which saved the lives of numerous animals and spared both the time and labour of the experimenters.

I hope you will forgive me for this long digression on the significance of surgical methods in physiology. I am sure that only the development of inventiveness and skill in performing operations on the digestive canal will disclose the magnificence of the chemical activity of this organ, some features of which can already be traced with the help of modern methods. I would ask you to remember these words at the end of my lectures, and I am convinced you will admit their truth.

LECTURE EIGHT

PHYSIOLOGICAL FACTS, HUMAN INSTINCT AND MEDICAL EMPIRICISM

Gentlemen,

The object of our discourse today is to consider the previously communicated results of our laboratory research from the point of view of the regulations relating to the partaking of food and of the therapeutic measures prescribed by the physician in disorders of the digestive apparatus. In the latter case, in order to perfect our knowledge and to secure its most useful practical application, the pathology and therapy of the digestive canal should, of course, be subjected to experimental investigation by the same methods and from the same point of view. Such inves-

tigation would now present no great difficulty; thanks to the progress of bacteriology many pathological processes can easily be produced in the laboratory, especially since in this case we are dealing, as it were, with external diseases: the methods used nowadays give access to any part of the surface of the digestive canal. On such pathological animals it would be possible precisely and thoroughly to study the functional disturbances of our apparatus, i.e., the changes which take place in the secretory activity—in the properties of the fluids and the conditions under which they are secreted. Therapeutic methods, too, could be experimentally tested on such animals by observing the entire course of healing and its final effect, i.e., by investigating the conditions of secretion during all phases of the healing process. It can hardly be doubted that only the development of experimental therapy, together with experimental physiology and pathology, can ensure for scientific, i.e., ideal, medicine its rightful position. Incontestable proof of this is provided by bacteriology, which has recently come into being and developed.

I have already described pathological therapeutic experiments with dogs whose vagi nerves were severed at the neck. I recall some other cases of a similar nature. At times our dog with the two stomachs suffered from a slight and usually transient gastric catarrh. It was very interesting to observe that the pathological disturbance caused by us in the large stomach made itself felt in the small one; the latter showed an almost continuous slimy secretion of very low acidity, but of strong digestive power. At the beginning of the illness or before it manifested itself, the psychic stimulation was strikingly effective—it furnished juice in normal quantity, while the local stimuli were almost completely ineffective. In this case one can assume that the deeper layers of the mucous membrane with the gastric glands still remained healthy, being easily excited to activity from the centres, while the surface of the mucous mem-

brane with the peripheral apparatus of the reflex nerves was already considerably injured. I mention these, one might say, impressions rather than precise facts, for the purpose of showing the fruitful field awaiting any investigator who undertakes to study, with the help of modern methods and practice, the pathological states of our digestive organs and their treatment. Such a study is all the more desirable since clinical investigation of this same subject, despite big efforts in recent years, meets, of course, with serious difficulties. It should be borne in mind that the stomach-probe, the chief clinical instrument, is less convenient than the gastric fistula practised on animals; and yet we know that the physiology of the stomach, even after years of application of the latter method, has not made any serious advance. This is understandable. We had before us a mixture of substances comprehension of which was difficult and at times even impossible.

And so a strictly scientific solution of therapeutic problems is still a matter for future research. But this in no way excludes the possibility of fruitful influence being exerted by the latest physiological achievements on the work of the physician. Physiology, of course, cannot lay claim to authoritative guidance in the field of medicine; because of the incompleteness of its knowledge it is always more restricted than clinical reality. But physiological knowledge often elucidates the mechanism responsible for one or other illness and the intrinsic significance of appropriate empirical methods of treatment. To apply a remedy without knowing how it will act is one thing and to know what you do is another, ensuring immeasurably greater advantages. In the latter case the influence on the affected organ will, of course, be more effective and more adapted to the given conditions. Besides, medicine, being constantly enriched with new physiological facts, will, sooner or later, become what in the ideal sense it should be, namely, the art of repairing the damaged mechanism of the human body

on the basis of exact knowledge; in other words, it will become applied physiology.

Let us return now to our basic subject. While it is generally recognized that human instinct is the result of everyday experience, which has turned into an unconscious striving for the best possible conditions of existence, in the physiology of digestion especially the phrase has become current that physiology merely confirms the rules of instinct. It appears to us that the foregoing physiological facts also furnish numerous instances of the triumph of instinct before the tribunal of physiology. Particularly impressive are the reasons underlying the daily empirical demand that food be enjoyed and eaten with relish. Everywhere the act of eating is connected with certain customs designed, as it were, to distract from the routine of daily life: a special time of the day is chosen; a group of relatives, acquaintances, or companions assemble; certain preparations are made (change of garments, as, for example, in Britain, grace is said by the oldest member of the family, etc.). In well-to-do houses there are special rooms for meals; musicians and others are invited to entertain the diners—in short, everything is done to distract the company from the cares and worries of daily life and to concentrate on the food. From this point of view it is also obvious why serious conversation, as well as serious reading, are considered inappropriate at meals. This, perhaps, partly explains also the use of alcoholic beverages at meals, since alcohol already in the early phases of its action has a mild narcotic effect which contributes to distraction from the burden of everyday cares. It goes without saying that this highly developed hygiene of eating is met with predominantly in the intelligent and well-to-do classes, first, because here mental activity is more strenuous and the varied questions of life are more disturbing; and secondly, because food is usually served here in quantities in excess of the requirements of the organism. In the lower classes, where mental activity is of a more elementary nature, highly strained muscular activity

and chronic insufficiency of nourishment normally evoke a strong and lively desire for food, without special measures for stimulating it. These conditions explain why the choice of food is so dainty in the case of the upper classes and can be so simple and at the same time harmless in that of the lower classes. All the condiments and all the appetizers used before a substantial repast are obviously designed to provoke curiosity, interest and a greater desire for food. It is a well-known fact that a person who at first displays indifference to his customary meal, afterwards begins to eat with gusto if his taste has been stimulated by something piquant. Thus, it is only necessary to give an impulse to the taste organs, to set them into action, in order that their further activity can be maintained by less powerful stimulants. Naturally, a person who is hungry does not need these extraordinary stimulating measures, since satisfying hunger is in itself a matter of pleasure. It is often said and not without reason that "hunger is the best sauce." However, here, too, it is a matter of degree, since every normal individual, even every animal, must feel a certain appetizing taste. For example, a dog that has gone without food for many hours eats not all the things that dogs usually eat, but chooses the food it likes best. Hence, the presence of certain flavouring substances in the food is a general requirement, although naturally individual tastes greatly differ. On the other hand, an extravagant indulgence of the appetite for food, like any other over-indulgence, is easily understood (for example, Petr Petrovich Petukh in Gogol's *Dead Souls*, and other gluttons).

This passing description of the attitude towards the act of eating testifies that people always take care to maintain attention to and interest in food, to ensure that meals are enjoyed; in other words, they take care of what is generally called appetite. Everybody knows that food eaten with appetite and pleasure is normal and useful food; food eaten to order, or for the sake of convenience, becomes to a greater or lesser degree, harmful, and the instinct of

human health acts against it. Hence, one of the most frequent requests addressed to the physician is to restore the lost appetite. In compliance with this, medical men at all times and in all countries until recently considered it important, in addition to combating fundamental disease, to take special measures for restoring appetite. It can be assumed that in this they were guided not only by the desire to free their patients from an unpleasant symptom, but also by the conviction that restoration of appetite facilitates the re-establishment of normal digestion. One can say that in the same measure as the patient wanted his appetite back the physician did all in his power to satisfy him. Hence the abundance of so-called remedies for restoring appetite. Unfortunately, modern medical science has considerably deviated from this correct and practical approach to appetite. It is astonishing how little attention modern text-books on digestive disorders devote to appetite as a symptom, and to its special therapy; only in some of them is the significance of appetite as a factor of the digestive activity mentioned in passing—in one or two parenthetical phrases. At the same time one comes across books in which the physician is practically advised not to treat bad appetite on the alleged grounds that it is an unimportant subjective symptom. From what I have said and demonstrated in previous lectures this attitude of modern medicine towards appetite cannot but be regarded as a serious misconception, since it is precisely here that symptomatic treatment is to a considerable degree concurrent with fundamental treatment. When in most cases of digestive disorders the physician finds it useful to stimulate secretory activity in every possible way, this aim can be achieved in the surest and most complete way by restoring the appetite. We have seen already that no other stimulant of gastric secretion can, in the matter of quantity and quality, compare with the craving for food. To a degree we can understand—and this helps to elucidate the matter—why modern medical science is so indifferent to loss of appetite

as an object of treatment. Nowadays, with the experimental method penetrating deeper and deeper into medical science, many factors of complicated pathological states and therapeutic agents are appraised, so to speak, according to the attestation of the laboratory, i.e., in so far as they can be verified in laboratory conditions. Of course, the highly progressive significance of this trend is beyond doubt; but here, as in any other human undertaking, error and exaggeration are inevitable. It should be borne in mind that if one or other phenomenon cannot be reproduced in laboratory conditions, this is no reason for discarding it as fantastic; we do not know as yet all the actual conditions for the development of certain phenomena, nor are we able to grasp the complex connection between separate vital functions. Seeking support in the laboratory, but unable to find there anything bearing on appetite, the clinic and the pathology of digestion, naturally, lost interest in this factor and disregarded it in medical practice. As already stated, until recently the psychic secretion of gastric juice was mentioned in physiology only in passing, and even then not by all authors, and rather as some kind of curiosity. On the other hand, great importance was attached to mechanical stimulation, the effect of which, now that our knowledge is more complete, has proved illusory. This error committed by physiology has been experimentally disclosed and explained; each of the contending agents has been assigned its proper place, and if clinical medicine follows its rightful desire to investigate its problems experimentally, it is obliged in practice to accord to appetite its right to consideration and treatment.

Despite the above-mentioned indifference displayed by physicians to appetite, so to speak, per se, many medical methods even now are, in essence, based on stimulation of appetite. Such is the truth of empiricism! When the patient is directed to eat sparingly and to avoid over-eating, when he is advised to abstain from eating pending the special permission of the physician, when he is removed from his

usual surroundings (according to Mitchell's method) or sent to a watering place, where life is riveted on the observance of certain physiological functions, and especially eating—in all these cases the physician is actually endeavouring to stimulate the patient's appetite and use it for curative purposes. In the first case, when the patient is counselled to eat sparingly, besides preventing a weak stomach from being overloaded, there undoubtedly takes place a recurrent secretion of appetite juice which is particularly profuse and strong in digestive power. I ask you to recall the previously described experiment in which the food given to a dog in small portions led to the secretion of a much stronger juice than a large portion eaten at once. This was an exact experimental reproduction of the clinical treatment of a weak stomach. This kind of diet regulation is all the more expedient since, in most frequent disorders of the stomach, only the surface layer of its membrane is affected. Thus the sensory surface of the stomach receiving the effect of the chemical stimulant, may not be able, so to speak, to cope with its duty, and the period of chemical stimulation of the gastric juice, which, with an abundant intake of food lasts for a long time, is for the most part deranged or even completely absent. Meanwhile a strong psychic excitation, a keen feeling of appetite, may be freely transmitted from the central nervous system to the gastric glands located in the deeper, yet unaffected layers of the mucous membrane. An example of this, taken from our laboratory pathological material, was mentioned by me at the beginning of the lecture. It is obvious that in these cases the surest way is to promote digestion only by exciting a secretion of appetite juice, and not juice excited by chemical stimulants. From this point of view the importance of removing a patient, suffering from chronic weakness of the stomach, from his customary surroundings, becomes quite clear. Let us take, for example, a mentally overstrained person during office hours. How often does it happen that he is unable to divert his thoughts from his

work for a single moment. He eats, as it were, without noticing it, without interrupting his work. This is quite common with people in cities where life is extremely strenuous. Naturally this systematic inattention to the act of eating leads, sooner or later, to digestive disorders, with all their consequences. There is no appetite juice, no igniting juice, or at least very little. The secretory activity develops very slowly. The food remains in the digestive canal for a much longer time than it is normally required; due to the insufficiency of digestive juices it is subject to fermentation and, in this state, greatly irritates the mucous membrane of the digestive canal. Thus the latter is brought naturally and gradually to a state of disorder. No prescriptions on the part of the physician can help the patient so long as he remains in his old surroundings, since the fundamental cause of his complaint has not been eliminated. There is only one solution—remove the patient from his everyday surroundings, free him from his usual work, interrupt the train of persistent thought and for a time concentrate his attention exclusively on the care of his health, on eating. This is done by sending the patient on a tour, to a watering place, etc. The duty of the physician is not simply regulating the behaviour of individual patients in this respect, but taking care that a proper attitude towards the process of eating be widely established. This is the duty especially of Russian physicians, because among the so-called intelligent classes in Russia with their highly confused conceptions of life, generally speaking, there is often an absolutely unphysiological, and sometimes even scornful indifference towards eating. More methodical nations, like the English, have made the act of eating something of a cult. If gluttony is looked upon as a manifestation of bestiality, a scornful indifference towards eating is, on the contrary, a manifestation of imprudence. As is always the case, the best course lies between the two extremes—one must not over-indulge in eating, but at the same time proper attention should be

devoted it. Render unto Caesar the things that are Caesar's, and unto God the things that are God's.

With the firmly established fact that mental influence is exerted on gastric secretion, the question of flavouring substances enters on a new phase. Now we know why the empirical conclusion was drawn that food must be tasty as well as nutritious. And therefore the physician, who prescribes diets for individuals, or even for groups of people, must constantly bear in mind the phenomenon of psychic secretion, i.e., he is obliged to inquire and know how the given food has been eaten—whether with or without relish. In reality, however, those responsible for diets often focus their attention exclusively on the nutritive value of the food or are guided solely by their own taste. It is impossible, in the interests of public health, not to call attention also to the feeding of children. If taste determines the individual's attitude towards food—and this evokes the initial activity of the digestive glands—then it would be irrational from the point of view of vitality to accustom children solely to delicate and uniform gustatory sensations; this would but reduce their further adaptability to the conditions of life.

The question of the therapeutic importance of bitters seems to me to be very closely connected with appetite. After enjoying a very long period of high repute these substances have been all but excluded from the list of pharmaceutical remedies. Laboratory tests showed that they were unable to live up to their old reputations; when introduced directly into the stomach or into the blood, many of them failed to produce the secretion of digestive juices; they thereby became greatly discredited in the eyes of clinicians, so much so that some of them were quite ready to abandon them altogether. Obviously, their fate was determined by the simple conclusion that a weakened digestion could be aided only by a remedy that would excite secretory activity under given conditions. It was, however, overlooked that the experimentally tested conditions could not cover all the

possible conditions of the process under investigation. The whole question of the therapeutic importance of bitters acquires quite a different aspect when linked with another question, namely, what is the effect of bitters on the appetite? The unanimous verdict of both the earlier and later physicians is that bitters definitely stimulate appetite; and that says everything. It follows then that bitters really stimulate secretion, since appetite, as has been frequently pointed out in these lectures, is the strongest stimulus to the digestive glands. There is nothing surprising in the fact that this was not observed in previous laboratory experiments. Bitters were injected directly into the stomach, and even into the blood of absolutely normal dogs. But their action is mainly bound up with the influence they exert on the gustatory nerves; and it is not accidental that this wide variety of remedies, covering substances of most diverse chemical composition, is grouped together chiefly because of a common bitter taste. The taste of a person suffering from digestive disorders is a blunted taste and displays a certain gustatory indifference. Ordinary food, agreeable to other people, and to himself when in health, is now tasteless; it not only does not excite appetite, but, on the contrary, rather evokes a feeling of distaste. In such patients the gustatory sensations, as it were, completely vanish or become distorted. Consequently, a powerful impulse should be given to the gustatory apparatus in order to restore strong and normal sensations. Experience has shown that this can be most quickly achieved by the application of sharp, unpleasant stimulants, which, by contrast, awaken the idea of pleasant gustatory sensations. In any case the indifference disappears and this makes it possible to excite appetite for one or another kind of food. This phenomenon reproduces a general physiological fact: light appears brighter after darkness, sound louder after silence, the joy of health more intense after illness, etc. This explanation of the stimulating action of bitters on the appetite proceeding from the mouth cavity does not exclude similar

action from the stomach cavity. As pointed out in the fifth lecture, there are grounds for assuming that certain stimuli coming from the stomach also excite appetite. It is possible that bitters, besides acting on the gustatory nerves in the cavity of the mouth, produce a peculiar effect on the mucous membrane of the stomach. This engenders certain sensations—separate elements of the craving for food. The fact that these special sensations arise in the stomach after the administration of bitters has been confirmed by some clinicians. Consequently, this must be a matter not merely of a simple physiological reflex, but of a certain psychic effect which causes the physiological secretory activity. The same, apparently, holds good for other substances, such as spices, vodka, etc. In any case, irrespective of whether this explanation conforms to reality or not, the question of the therapeutic importance of bitters, I repeat, has been decided positively, since their obvious effect on appetite is generally acknowledged. And so experimental investigation of bitters has the job of establishing their influence on appetite, which is a difficult matter not yet attempted in the laboratory.

It is not sufficient, therefore, to verify the clinical observations in the laboratory on animals; one must have, in addition, the assurance that this verification is correctly carried out, i.e., that the investigation concerns the particular point of the process under clinical consideration. It is interesting to observe that the connection between appetite and gastric secretion is pictured in exactly the reverse way by many physicians and in many medical text-books; thus it is assumed that a certain therapeutic agent causes the secretion of gastric juice and that the presence of the latter in the stomach evokes appetite. Here we have, apparently, a wrong interpretation of a true fact, since what is overlooked is that a psychic effect could also be a strong excitant of the secretory nerves.

After one or other hors d'oeuvre or some vodka (especially customary in Russia) designed to excite the appetite, the

main meal in most cases begins with something hot, with meat broth (bouillon, different soups, etc.). This is followed by the really nutritious food—meat of different kinds served in different ways, or in the case of poorer people, porridges rich in starch and protein. This sequence of foods is quite natural from the point of view of the physiological facts mentioned in these lectures. As we have seen already, meat broth is an essential chemical stimulant of gastric secretion. Consequently, practical experience secures two ways of exciting a profuse flow of gastric juice on the fundamental food: first by stimulating the secretion of appetite juice with the help of the hors d'oeuvres, and secondly by promoting the gastric secretion with the help of meat broth. Human instinct has thus developed a preliminary procedure for the digestion of the main food. However, only well-to-do people can afford a good meat broth; poorer people use for the excitation of early gastric secretion cheaper and less effective chemical stimulants. Russians, for instance, use kvass;* in Germany, where meat is quite expensive, strictly speaking, a slightly flavoured and warmed up water is used (Mehlsuppe, Semmelsuppe, etc.). Probably not without significance is the fact that the quantity of digestive juice is, generally speaking, closely connected with the water content in the organism. If this sequence of food holds good for healthy people, then it is all the more obligatory in pathological cases. A person suffering from loss of appetite, or with a bad appetite, has no psychic secretion of gastric juice at all, or if he has, it is very feeble. Consequently, his meal must inevitably begin with a strong chemical stimulant, i.e., with a solution of stimulative meat extracts. Otherwise solid food, especially non-meat foods, would remain undigested for a long time. Hence, it is quite expedient to prescribe meat juice, strong bouillon or a solution of the Liebig extract for patients suffering from loss of appetite. The same applies

* A Russian beverage made of malt, water and different varieties of bread.—*Tr.*

to forcible feeding, for example, of mental patients. In the latter case, the very method of introducing the food ensures the supply of a chemical stimulant, since the food can be introduced only in liquid form; in any case the addition of the Liebig extract to liquid food substances would be very useful. According to the strength of the chemical excitation, the liquid substances may be arranged in the following order: first, the substances just mentioned (meat juice, etc.), secondly—milk, and thirdly, water.

The usual termination of a dinner can also be easily understood from the present physiological standpoint. A dinner usually ends with the sweet course, and everybody knows from experience that sweets give certain pleasure. The meaning of this is quite plain. A repast which begins with satisfaction evoked by the desire for food, must end with the same sensation, despite the fact that the hungry feeling has been satisfied, moreover, the object of this satisfaction is a substance that calls for practically no digestion but agreeably excites the gustatory organs, namely, sugar.

Having considered the general arrangement of eating from the point of view of physiological facts, we shall now turn to some special points.

First of all, I shall touch on the acid reaction of the food. It is obvious that of all tastes acid occupies a very special place. A number of acid substances are in use—one of the commonest being vinegar, which is used in the preparation of numerous sauces and dressings. Many wines are also acid in taste. In Russia, kvass, especially acid kvass, is widely consumed. Then, large quantities of acid fruits and vegetables are consumed, some of them acid in themselves and some being made acid in the course of preparation. Medicine, too, makes use of this instinct, and solutions of acids, mostly hydrochloric and phosphoric, are often prescribed in disorders of digestion. Finally, nature herself constantly takes care to produce in the stomach, during normal digestion, along with hydrochloric acid, also lactic

acid, which is formed from ingested food and, consequently, is always present. These facts have become physiologically comprehensible, because we know that an acid reaction in the digestive canal is not only required for the effective action of the chief gastric ferment, but is also a very strong stimulant of the pancreatic gland. It can even be assumed that in certain cases the entire digestion depends solely on the acid reaction (as a digestive stimulant), since the pancreatic juice has a fermentive action on all the components of the food. Thus, the above-mentioned acids now assist digestion, now serve as a remedy, and substitute the gastric juice when it is fully absent or relatively insufficient. From this point of view it is not difficult to understand why a close combination of kvass and bread is widely used by Russian peasants: the enormous quantity of starch consumed by them in the form of bread or porridge necessitates strong excitation of the pancreatic gland, and this is opportunely effected by the acid. In isolated stomach complaints, accompanied by loss of appetite, both instinct and medicine have recourse to acids, since, as we now know, they excite intensified activity of the pancreatic gland and thereby supplement the insufficient work of the gastric glands. In my view this knowledge of the special relation of acids to the pancreatic gland may be of great use to practical medicine, and may place the pancreatic gland, this powerful and important organ of digestion hidden so deep in the organism, under the strict control of the physician. It is possible, for instance, to exclude the stomach deliberately from the digestive process and transfer digestion direct to the bowel by prescribing acids that do not stimulate the gastric glands. It is possible, likewise, to restrict the activity of the pancreatic gland by reducing the acidity of the gastric contents. This may be necessary in treating various digestive disturbances, as well as certain general complaints.

No less instructive is a comparison of our experiments on fats with the demands of instinct and the counsel of dietetics and therapy. It is generally recognized that fatty

food is heavy, i.e., difficult to digest; in cases of weak stomachs it is, therefore, usually avoided. Now we are able to explain this physiologically. The fat, when present in large quantities in the chyme, retards, in its own interest, the secretion of gastric juice and thus impedes the digestion of protein substances. This explains why a combination of fat and protein foods is particularly heavy, and only strong stomachs and persons with keen appetites can cope with them. A combination of bread and butter proves less difficult of digestion as can be judged from its widespread use. Bread, as we have already seen, requires little gastric juice and little acid, especially when calculated per unit of time; but the fat which excites the pancreatic gland ensures a simultaneous production of ferment for itself and for the starch and protein. Fat alone is by no means a heavy food, as is proved by the fact that large quantities of Ukrainian bacon can be consumed with impunity. This is understandable, because in this case the inhibitory action of the fat on the secretion of gastric juice is harmless; it contributes merely to assimilation of the fat itself. There is no conflict between the food components and consequently no one of them suffers. Fully in accordance with practical experience, the physician completely excludes fatty foods from the diets of patients in cases of weakness of the stomach and recommends red meat only, for example, game. But in those pathological cases where there is excessive activity of the gastric glands, fatty food or fat in the form of an emulsion, is prescribed by the physician. In this case, obviously, medicine has empirically learned to make use of the inhibitory action of fat on the gastric secretion, so strikingly manifested in the foregoing experiments on dogs.

Milk has an exceptional place in human food, and this is recognized both in everyday experience and in medical practice. It has always been regarded as the lightest food and is given in cases of weak and diseased stomachs, as well as in numerous other severe illnesses, such as heart

and kidney affections. The extreme importance of milk as a food prepared by nature herself has now been elucidated to a considerable degree. We can indicate three essential properties of milk which characterize it as an exceptional food. As we already know, in comparison with the nitrogenous equivalents of other foods, the weakest gastric juice and the smallest quantity of pancreatic juice are poured out on milk. Thus, the secretory activity required for the assimilation of milk is considerably weaker compared with any other food. At the same time milk has another important property. When introduced directly into the stomach without the animal noticing it, the milk always causes a certain degree of secretory activity of the stomach and the pancreas, i.e., it acts as an independent chemical stimulant of the digestive canal; and it is really a mysterious phenomenon that no essential difference is observed between the secretory activity caused in the digestive canal when the milk is introduced unnoticed into the stomach and that which arises when the milk is ingested by the animal. In the case of meat, as we already know, the mode of introduction into the stomach is of extreme importance, even though meat is a better chemical stimulant. Consequently, it must be assumed that milk itself causes not only an absolutely adequate, but also a highly economical secretion, and not even appetite can make this secretion more abundant, so to speak, more luxuriant. Unfortunately, the secret of this specific relation of milk to the secretory activity of the digestive canal cannot yet be analysed and explained. It may be supposed that on the one hand, a certain role is played here by the fat which inhibits the activity of the gastric glands, and, on the other hand, by the alkaline reaction of the milk which inhibits the activity of the pancreatic gland. Thus, both the gastric glands and the pancreas, despite the presence of stimulants in the milk, are maintained at a certain, not too high, level of activity, and this in its turn appears expedient in view of the easy digestibility of all the components of milk. Finally, the third characteristic

property observed in milk, and which in all probability is only another expression of the first, is seen in the following. If we feed the animal with equal quantities of nitrogen, in one case in the form of milk, in the other in the form of bread, and then observe the hourly excretion of nitrogen in the urine, we find that the increase during the first seven to ten hours after the administration of milk (compared with the rate of excretion beforehand) comprises only from 12 to 15 per cent of the nitrogen taken in with the milk, whereas in the case of bread it rises to 50 per cent. Taking into consideration the course and rate of assimilation of milk and bread, one must admit that the increase in urinary nitrogen, which takes place immediately after eating, expresses the functional intensity of the working metamorphosis of the alimentary canal required for digestion, and that in the case of bread this intensity is three or four times greater than in the case of milk (Prof. Ryazantsev's experiments). Hence, in the case of milk a considerably larger portion of nitrogen is, so to speak, placed at the disposal of the organism than in that of any other kind of food. In other words, the price paid by the organism for the nitrogen (in the form of the work of the digestive canal) is much lower than that for nitrogen in other food substances. How wonderfully does the food prepared by nature distinguish itself compared with the other foods! These facts, obviously, pose the question of a new approach towards the comparative nutritive value of the different foods. The old criteria must give way to new, or, to be more precise, admit the latter to their midst. Experiments on the assimilation of foods, with the aim of ascertaining what remains undigested and what enters the organism's juices, in themselves cannot solve the question in a satisfactory way. Suppose that we set the alimentary canal a certain task connected with the digestion of a given food. If it is sound, this task will be accomplished in the best possible way, i.e., with complete extraction of all the nutriment. In this way we would learn how much nutritive material is contained in the given kind

of food in general, but the question of the digestibility of the food would still be obscure. The experiment does not disclose the magnitude of the effort of the digestive canal in extracting all the nutritives from the given food. Nor can experiments on artificial digestion fully solve the question of digestibility, since experiments with food normally ingested are quite different from those in the test tube, where we deal only with one juice, in the absence of any interaction with other juices and food components. That an essential distinction really exists here is clearly proved by the investigations made in our laboratory by Dr. Walther. Fibrin, which is generally considered the most digestible protein, when compared with milk of the same nitrogenous equivalent, proved to be a much stronger stimulant of the pancreatic gland, while apart from nitrogenous substances milk contains a good deal of non-nitrogenous nutritive material. It is obvious, therefore, that the question of digestibility and nutritive value of foods must be solved mainly by means of estimating the real energy used in the process of their digestion, i.e., the quantity and quality of the juices poured out on the given amount of nutritive material. The energy of the glandular metamorphosis must be deducted from that of the entire intake of food, the remainder will indicate the extent to which the food is utilized by the organism, i.e., the amount available for use by all the organs, with the exception of the digestive apparatus. From this point of view those food substances, the bulk of which is used to compensate for the expenditure by the alimentary canal on their digestion, must be regarded as not very nutritious and as indigestible, in other words, as foods whose nutritive value covers only the cost of their own digestion. It is, therefore, of great practical importance that one and the same kind of food but differently served should be compared from this aspect, for example, boiled and roast meat, hard- and soft-boiled eggs, boiled and unboiled milk, etc.

It remains for me now to touch on some purely medical questions. The first concerns the therapeutic administration of neutral and alkaline salts of sodium. Clinical, pharmacological and physiological text-books have always advanced as a well-grounded doctrine the thesis that these salts evoke a secretion of juice. However, we would search in vain for any serious experimental backing for this doctrine. The experimental facts adduced cannot be considered satisfactory. The experiments carried out by Blondlot who sprinkled sodium bicarbonate on meat, as well as the experiments of Braun and Grützner who injected solutions of sodium chloride direct into the blood, either suffered from methodological defects or were far removed from normal relations. We can hazard the guess that in this case the experimental insufficiency was benevolently made good by the clinic, since the experiment seemed to confirm the clinical observations. There is no doubt, of course, that salts of sodium (the chloride and bicarbonate) are useful in digestive disorders. But how do they act? It appears to me that here, as in some other similar cases, medical thought has fallen into error, since the effect of the action is one thing, and its mechanism—altogether different. Although medicine is broad and comprehensive in its empiricism, it often manifests narrow reasoning when it comes to interpreting the facts; it often gives a simplified explanation of the highly complex mechanism of healing processes merely on the basis of modern physiological data. It seems to me that this is also true in the given case. The following reasoning is current in medicine: "alkalies act favourably in disturbances of the digestive canal; consequently they provoke a flow of digestive juices." Of course, upon recovery the stomach begins to secrete the normal, i.e., in some cases a larger, quantity of gastric juice. But this must be the result of recovery from a disordered state and not of the direct physiological effect of the alkalies. The latter, however, should be thoroughly, that is, specially proved. The help given to the organism by the alkalies might be explained

in another way, altogether different from the ordinary. In this case I venture to express the idea of the therapeutic effect of sodium chloride and of alkaline sodium salts, which is the exact opposite of the generally accepted idea. Both on the stomach and the pancreas we failed to convince ourselves of any juice-exciting influence of these salts; on the contrary, in our hands they exerted an inhibitory influence on secretion. In addition to the previously described experiments concerning the action of alkalies on the stomach and pancreas, I should like to mention the following observation. A dog which had been subjected to complex operations—a gastric fistula, a pancreatic fistula and an oesophagotomy—was given daily, for a period of weeks, an addition of soda to its food. The animal had a perfect appetite and was in good health. When we performed the first experiment with sham feeding, we were struck by the relatively weak effect of this generally powerful juice-exciting procedure. At the same time we observed that the pieces of meat which fell out of the upper end of the oesophagus contrary to the usual rule, had hardly been touched by the saliva. Consequently, this dog exhibited reduced activity of many digestive glands simultaneously, namely, of the gastric, pancreatic and salivary glands. The work of the salivary glands, of course, deserves closer investigation. I believe that the experimentally proved inhibitory action of alkalies on the digestive glands gives grounds for advancing the following concept of the mechanism of their healing effect in certain digestive disorders. Catarrhal complaints of the stomach are characterized by a constant or greatly protracted secretion of slimy gastric juice of extremely low acidity. Moreover, in certain cases the affection begins with hypersecretion, with abnormal excitability of the secretory apparatus manifested in an excessive and causeless flow of gastric juice. The same thing can be assumed in the case of disorders affecting the pancreatic gland, judging by its state after operations performed on it for physiological purposes. It can be supposed that when for some

reason or other the above-mentioned affections arise, they afterwards, so to speak, maintain themselves independently, since continuous activity is obviously injurious to the glands. The nourishment and restoration of the glandular organs proceeds best during rest; such is the normal course —after a period of external work there comes a break, followed by a period of internal work. Consequently, the elimination of the pathological state and the return to normal can be attained by means of a remedy which forcibly interrupts the external work of the affected glandular organ. Such, in my opinion, is the healing importance of the alkalies. One might draw a certain parallel between the effect of the alkalies in a pathological state of the digestive canal and that of digitalis in compensatory disturbances of the heart. A heart so affected usually beats rapidly, thereby aggravating its condition, shortening the period of its rest, i.e., of its recovery. A circulus viciosus sets in; the weak work of the heart reduces the blood pressure; in view of the constant physiological connection this leads to a quickening of the heart-beat, which, in its turn, causes a further weakening of the heart. Undoubtedly the favourable action of digitalis begins to manifest itself in the fact that it breaks through this vicious circle, slows the pulse and in this way gives new strength to the heart. Our explanation of the action of the alkalies accords with the usual practice of prescribing a strict diet simultaneously with their use, which ensures a certain rest for the digestive glands. It is interesting to note that clinical investigations with the help of the stomach tube, after a period when the alkalies were regarded as having a juice-exciting effect, recently entered into a new phase and that more and more is heard of the inhibitory effect of the alkalies.

The second point that we should consider is the following. The main difficulty encountered by the physician in determining the diet of patients suffering from digestive disorders is that a very important role is played here by idio-

syncrasy. In cases of one and the same illness different patients react to the same kinds of food in an absolutely different manner. That which is agreeable to one patient, and which is easily borne by him and eases his condition, is almost poison for another patient. In one of the clinical manuals it is stated that while some patients easily bear milk and will not have fatty goose flesh, others have a reverse reaction to the same food. Hence the first and foremost rule in dietetics is to give no instructions with regard to diet until the taste and habits of the patient have been ascertained. What does this signify? Until recently physiology had no experimental answer to this question. But our facts, it seems to me, to a certain degree clarify the matter. Each kind of food leads to certain digestive activity, and a continuous diet sets up definite and fixed types of glands which cannot be quickly and easily altered. This explains why digestive disorders often arise when there is a sudden change from one diet to another, especially from a frugal to a richer diet, as is the case, for example, after the long Russian fasts; these disorders are manifestations of the temporary inability of the glands to adapt themselves to the new digestive task.

Finally, it may be useful to mention the following. There are cases of very acute and, as it were, absolutely unjustified disturbances of the digestive canal. From the point of view of modern physiology these cases might be explained by interference of the secreto-inhibitory nervous system arising from excessive and abnormal excitation due to one or another cause. In any case this system now represents a factor which must be duly considered by the physician.

At this point, gentlemen, I conclude my lectures. I hope that the physiological facts communicated here, may help the physician to understand certain things in his sphere of activity, and that they will contribute to the elaboration of more correct and effective methods of treatment. The physician will derive more benefit by bringing to the attention

of the physiologist in what way, in his opinion, the explanations given here need correction, and also by pointing out those new aspects of the digestive process which have been disclosed by him in the broad world of clinical observation but which have not yet come within the field of vision of the physiologist. It is my profound belief that the aims of physiology as a branch of knowledge and of medicine as an applied science can be attained solely by means of active exchange of experience between physiologist and physician.

NOBEL SPEECH DELIVERED IN STOCKHOLM ON DECEMBER 12, 1904[25]

It is not accidental that all the phenomena of human life are dominated by the matter of daily bread—the oldest link connecting all living things, man included, with the surrounding nature. The food which finds its way into the organism where it undergoes certain changes—dissociates, enters into new combinations and again dissociates—embodies the vital process, in all its fulness, from such elementary physical properties of the organism, as the law of gravitation, inertia, etc., all the way to the highest manifestations of human nature. Precise knowledge of what happens to the food entering the organism must be the subject of ideal physiology, the physiology of the future. Present-day physiology can but engage in the continuous accumulation of material for the achievement of this remote aim.

The first stage through which the food substances introduced from without must pass, is the digestive canal; the first vital action on these substances, or to be more exact and objective, their first participation in life, in the vital process, is effected by what we know as digestion.

The digestive canal is a kind of tube passing through the entire organism and communicating with the external world, i.e., also on external surface of the body, but turned inwards and thus hidden in the organism.

The physiologist who succeeds in penetrating deeper and deeper into the digestive canal becomes convinced that it consists of a number of chemical laboratories equipped with various mechanical devices.

These mechanical devices are formed by the muscular tissue which is a constituent part of the wall of the digestive canal. They either facilitate the passage of the components of food from one laboratory to another, or detain them for a certain time in a given laboratory or, finally, expel them when they prove harmful to the organism; moreover, they participate in the mechanical processing of the food, accelerating the chemical action on it by compact mixing, etc.

A special so-called glandular tissue which is either also a constituent part of the wall of the digestive canal, or lies beyond it in the shape of separate masses and communicates with it by means of branch tubes, produces chemical reagents, the so-called digestive juices which stream into separate segments of the digestive tube. The reagents, on the one hand, are aqueous solutions of such well-known chemical substances as hydrochloric acid, soda, etc., and, on the other hand, substances which are found only in a living organism and which break up the main components of food (proteins, carbohydrates and fats) much more easily, i.e., more rapidly and at a much lower temperature and in smaller quantities than any other chemically well-studied substances. These substances which act in vitro[26] just as well as in the digestive canal, and which, therefore, are a natural object for chemical investigation, have so far been difficult to analyse. As is known, they are called ferments.

From this general description of the digestive process I shall turn to facts relating to this process established by me and by the laboratory of which I am in charge. In doing so I deem it my duty to recall with profound gratitude my numerous laboratory co-workers.

It is perfectly clear that successful study of the digestive process, as of any other function of the organism, depends to a considerable degree on whether we succeed in finding the nearest and most convenient starting-point in relation to the process under observation and on whether all col-

lateral processes between the phenomena under observation and the observer are removed.

For the purpose of investigating the development of secretion in the big digestive glands, which communicate with the digestive canal only by means of branch tubes, we cut from the wall of the digestive canal small pieces, in the centre of which were the normal openings of the secretory ducts; we then stitched the opening in the wall of the canal, and the excised pieces with the openings of the secretory ducts were sutured to a corresponding place on the surface of the skin, from the outside. Thanks to this procedure the juice was diverted from the digestive canal and collected in special vessels. For the purpose of collecting the juice produced by the microscopic glands located directly in the wall of the digestive canal, already long ago large pieces were cut out from the wall of the digestive canal and artificial pouches with openings to the outside were made; the defect in the digestive canal, naturally, was closed by stitching. In the case of the stomach the preparation of the artificially isolated pouch was always connected with sectioning the nerves of the glandular cells, and this, naturally, deranged the normal work of the stomach.

Taking into account more delicate anatomical relations, we modified the operation in a way that left the normal nervous paths fully intact when making an isolated pouch from parts of the stomach wall.

Finally, since the digestive canal is a complex system, a number of separate chemical laboratories, I used to cut the communication between them in order to investigate the course of phenomena in each particular laboratory; thus I divided the digestive canal into several separate parts. This, of course, necessitated laying short and convenient passageways from the outside into each separate laboratory. For this purpose metal tubes have long been in use; they are inserted into the artificial openings, and during the intervals between the experiments they can be sealed.

In this way we often performed very thorough opera-

tions and sometimes even several operations on one and the same animal. It goes without saying that the desire to accomplish the task with more confidence, to avoid wasting time and labour, and to spare our experimental animals as much as possible, made us strictly observe all the precautions taken by surgeons in respect to their patients. Here, too, we had to apply proper anaesthesia, observe irreproachable cleanliness during the operation, provide clean rooms after the operation, and take thorough care of the wounds. But these measures did not suffice. After remaking the animal's organism in accordance with our design, which naturally caused more or less damage to the experimental animal, we had to find a modus vivendi that would ensure an absolutely normal and long life for it. Only by observing this condition would the results of our work be regarded as fully conclusive and as having elucidated the normal development of the phenomena. We succeeded thanks to our correct appraisal of the changes evoked in the organism, and thanks to the expedient measures taken by us; our healthy and happy animals did their laboratory work with real gusto; they always rushed from their cages to the laboratory and readily jumped on to the tables where our experiments and observations were conducted. Believe me I am not exaggerating one iota. Thanks to our surgical methods in physiology we can demonstrate at any time phenomena of digestion without the loss of even a single drop of blood, without a single scream from the animal undergoing the experiment. At the same time this is an extremely important practical application of the power of human knowledge, which may also be of immediate use to man, who, due to the implacable fortuities of life, is often mutilated in similar, though more diversified ways.

In our observations on dogs, we soon noticed the following fundamental fact: the kind of substances getting into the digestive canal from the external world, i.e., whether edible or inedible, dry or liquid, as well as the

different food substances, determined the onset of the work of the digestive glands, the peculiarities of their functioning in each case, the amount of reagents produced by them, and their composition. This can be proved by a number of facts. Take, for instance, the formation of saliva by the mucous salivary glands. With each meal, when edible substances find their way into the oral cavity, thick and viscous saliva containing much mucus flows out of these glands. With the introduction into the animal's mouth of substances that it finds offensive, such as salt, acid, mustard, etc., the saliva may flow in the same quantity as in the first case, but its quality is quite different—it is fluid and watery. If the dog is given now meat, now ordinary bread, then, other conditions being equal, the secretion of saliva in the second case will be more abundant than in the first. Similarly, some of the substances which are rejected by the animal, for example, such irritants as acid, alkali and others, evoke a more profuse secretion of saliva than other, chemically indifferent substances, like bitters; consequently here, too, different activity of the salivary glands is observed. The gastric glands react in the same way; they secrete their juice now in larger, now in smaller quantities, now of a higher and now of a lower acidity; its content of pepsin—a ferment dissolving protein—is sometimes greater, sometimes smaller. Bread evokes the secretion of gastric juice with the highest ferment content, but with a very low acid content; milk evokes the minimum ferment content, while meat evokes the maximum acid content. Under the action of certain quantities of protein introduced in the form of bread, the glands produce from two to four times as much protein ferment as in the case of meat or milk.

However, the diversity of the work of the gastric glands is not confined to the above-mentioned phenomena; it is manifested also in peculiar fluctuations in the quantity and quality of the reagents during the period of the functioning of the glands after the introduction of one or another food substance.

But that will suffice. I should only abuse your attention by giving an exposition of all the facts collected by us in this field. I shall merely remark that similar correlations were observed by us in the activity of all the other glands of the digestive canal.

Now it may be asked: what does this changeability in the work of the glands signify? In reply we shall revert to the phenomenon of salivary secretion. Edible substances evoke the secretion of thicker and more concentrated saliva. Why? The answer, obviously, is that this enables the mass of food to pass smoothly through the tube leading from the mouth into the stomach. Under the action of certain substances disagreeable to the dog the same glands secrete fluid saliva. What purpose does the saliva serve in such cases? Obviously, either to dilute these substances and thereby weaken their chemically irritating action, or, as we know from our own experience, to cleanse the mouth from such substances. In this case water, not mucus, is required, and the water is actually secreted.

As we have seen, bread, and especially dry bread, evokes secretion of considerably larger quantities of saliva than meat. This, too, is perfectly understandable: the eating of dry bread requires saliva, firstly, to dissolve the components of the bread and so make it possible to taste it (since something utterly inedible may get into the mouth!), and secondly, to soften the hard and dry bread, otherwise it would go down with difficulty and could even cause injury to the walls of the oesophagus while moving from the mouth to the stomach.

The relations inside the stomach are exactly the same. The bread protein induces secretion of more protein ferment than the protein of milk or meat, and a corresponding phenomenon is observed in the test tube: the protein of meat and milk is broken up by the protein ferment more easily than the vegetable protein.

Here again I could cite numerous additional examples of such expedient links between the work of the digestive

glands and the properties of the substances entering the digestive canal (but this I shall do later, if and when opportunity offers). There is nothing surprising in this phenomenon; and no other relations could be expected. It is clear to all that the animal organism is a highly complex system consisting of an almost infinite quantity of parts connected both with one another and, as a single complex, with the surrounding world, with which it is in a state of equilibrium. The equilibrium of this system, as of any other system, is an indispensable condition for its existence. And if in certain cases we are unable to disclose the expedient connections in this system, the reason is that we lack knowledge; it does not mean that these connections are absent in a system that has the quality of permanence.

Now we shall pass to another question which arises from what has been said above: how is this equilibrium effected? Why is it that the glands produce and secrete in the canal the reagents needed for the successful treatment of the respective object? Clearly, it should be admitted that in some way the definite properties of the object act on the gland, evoke in it a specific reaction and cause its specific activity. Analysis of this influence on the gland is an extremely intricate matter and one that requires much time. The main thing is to reveal in the object those properties which, in this particular case, act as *stimuli* on the glands in question. An investigation of this kind is not so easy as it looks at first sight. Here are some facts to prove this. By means of the previously mentioned metal tube, we introduce meat into the empty and inactive stomach of the dog, without the animal noticing it. In a few minutes the gastric reagent, a solution of the gastric protein ferment, begins to exude from the walls of the stomach. But which property of the mass of meat has acted as the stimulus on the gastric glands? The simplest way would be to assume that this action has been caused by its mechanical properties—pressure, or friction against the walls of the stomach. But such an assumption would be absolutely wrong. Mechanical in-

fluences are completely ineffective with regard to the gastric glands. We can mechanically influence the wall of the stomach in any way—strongly or feebly, continuously or with interruptions, on limited areas or in a diffused way—but without obtaining a single drop of gastric juice. Actually it is the components of meat dissolved in water that are the stimulating substances. However, as yet we lack sufficient knowledge of these substances since the extractive substances of meat form a vast group that still awaits investigation in full measure.

Here is one more example. A few minutes after the chyme finds its way into the nearest section of the digestive canal—into the duodenum—one of the glands of this section comes into action; this is the *pancreas*—a large organ located at the side of the digestive canal and connected with it by an excretory duct. But which of the properties of the chyme advancing in the bowels act as a stimulating agent on the gland? Contrary to our expectations, it turned out that this action was exerted not by the properties of the consumed food, but by the properties of the juice which joined it in the stomach, namely, by its acid content. If we pour into the stomach or directly into the intestine pure gastric juice, or simply the acid which it contains, and even some other acid, our gland will begin to function just as vigorously, or even more vigorously, than in the case of the normal chyme passing from the stomach into the bowels. The profound significance of this unexpected fact is quite clear.

The gastric laboratory uses its protein ferment under an acid reaction. Different intestinal ferments, and, among them, naturally, pancreatic ferments, cannot develop their activity in an acid medium. Hence, it is clear that the first task of the laboratory is to provide the neutral or alkaline reaction necessary for its fruitful activity. These relations are effected by the above-mentioned interconnections, since the acid content of the stomach, as already stated, induces secretion of alkaline pancreatic juice (and the higher the

acid content, the greater the secretion). Thus, the pancreatic juice acts first of all as a solution of soda.

One more example. It has been known for a long time that the pancreatic juice contains all the three ferments which act on the major food substances—the protein ferment, which is different from the gastric, the starchy ferment and the fatty ferment. As proved by our experiments, the protein ferment in the pancreatic juice is, constantly or at times, wholly or partly (this is still a matter of argument), in an inert, latent form. This can be explained by the fact that an active protein ferment might endanger the other two pancreatic ferments and destroy them. Simultaneously we established that the walls of the upper section of the bowels send into its lumen a special fermentative substance the purpose of which is to transform the inert pancreatic protein ferment into an active one. The active ferment, upon coming into contact with the protein substances of the food in the bowels, loses its noxious action with regard to other ferments. *The above-mentioned special intestinal ferment is secreted by the wall of the intestine due solely to the stimulating action of the pancreatic protein ferment.*

Thus, the expedient connection of phenomena is based on the specific properties of the stimuli and on the similarly specific reactions corresponding to them. But this by no means exhausts the subject. Now the following question should be put: how does the given property of the object, the given stimulant, reach the glandular tissue itself, its cellular elements? The system of the organism, of its countless parts, is united into a single entity in two ways: by means of the specific tissue which exists solely for the purpose of maintaining mutual relations, that is, the nervous tissue, and by means of tissue fluids which wash all the tissue elements. It is these very intermediaries that transmit our stimuli to the glandular tissue. We have thoroughly investigated the first of these interrelations.

Long before us it was established that the work of the salivary glands is regulated by a complex nervous apparatus. The endings of the centripetal sensory nerves are irritated in the oral cavity by different stimuli; the irritation is transmitted via these nerves to the central nervous system and thence, with the help of special centrifugal, secretory nerve fibres directly connected with the glandular cells, it reaches the secretory elements and induces them to certain activity. As is known, this process, as a whole, is designated as a *reflex* or as a reflex stimulation.

We have asserted, and we have proved experimentally, that normally this reflex is always of a specific nature, that is, that the endings of the centripetal nerves receiving the stimulation are different, each sending out a reflex only when there are definite external stimuli. Accordingly, the stimulus reaching the glandular cell must also be of a specific, peculiar character. This is a very profound mechanism securing the expedient dependence of the work of the organs on the external influences, and the connection which is effected with the help of the nervous system.

As was to be expected, the discovery of the nervous apparatus of the salivary glands immediately impelled physiologists to seek a similar apparatus in other glands lying deeper in the digestive canal. And despite the big expenditure of effort, positive results in this respect baffled researchers for a long time. Obviously the new objects of investigation had important properties which prevented the researchers, using the old methods, from disclosing anything.

Having taken into account these specific relations, we were able, fortunately, to achieve what for a long time had been regarded as a pium desiderium.[27] Physiology has, at last, gained control over the nerves which stimulate the gastric glands and the pancreas. Our success was mainly due to the fact that we stimulated the nerves of animals that freely stood on their feet and were not subjected to morbid irritation either during stimulation of their nerves or immediately before it.

Our experiments proved the existence not only of a nervous apparatus in the above-mentioned glands, but also disclosed facts clearly showing the participation of these nerves in normal activity. Here is a striking example.

We performed two simple operations very easily endured by dogs, and after which, if taken good care of, they live for years absolutely healthy and normal. The operations were as follows: 1) The oesophagus was severed at the neck and both ends separately sutured to the skin of the neck in a way that prevented food passing from the mouth into the stomach of the animal—it fell through the upper opening of the digestive canal; 2) a metal tube was introduced into the stomach through the abdominal wall—an operation mentioned earlier and already practised long ago. It will be understood that the animals were fed in a way that allowed the food to enter direct into the stomach through the metal tube. When, after a fast of several hours and after the empty stomach of the dog had been thoroughly washed, the animal was fed in the normal way (the food, as already mentioned, falling out of the oesophagus without reaching the stomach), in a few minutes the empty stomach began to secrete pure gastric juice. The secretion lasted as long as the animal was given food, sometimes even persisting long after the discontinuance of the so-called sham feeding. In these conditions the secretion of juice is very abundant, and it is possible to obtain in this way hundreds of cubic centimetres of gastric juice. In our laboratory we perform this operation on many dogs and the gastric juice thus obtained not only serves the purposes of research, but is also a good remedy for patients suffering from insufficient activity of the gastric glands. Thus a part of the vital supplies of our animals, which live for years (more than seven or eight years) without revealing even the slightest deviation from normal health, proves beneficial to man.

From the above-mentioned experiment it is clear that the mere process of eating, even when the food does not reach

the stomach, stimulates the gastric glands. If we sever at the neck of this dog the so-called vagus nerves, the sham feeding will not cause any secretion of gastric juice, no matter how long the dog lives and how well it feels. Thus, the stimulation produced by the process of eating reaches the gastric glands via the nervous fibres contained in the vagus nerves.

Now I shall take the liberty of deviating briefly from the main topic of my lecture. The severing of nn. vagorum is an operation that has been performed on animals for a long time and has always proved fatal. In the course of the 19th century physiologists studied the numerous influences exerted by the vagus nerves on the different organs, and their respective investigations revealed at least four disturbances which take place in the organism after the sectioning of these nerves, each of which is by itself of a lethal character. Appropriate measures were taken by us to prevent these disturbances in our dogs; one of these measures related to the digestive system; thanks to this the animals whose vagus nerves were severed enjoyed a healthy and happy life. Thus, four simultaneously acting lethal factors, were deliberately eliminated. Here we have striking proof of the power of science, which regards the organism as a machine!

Some ten years ago the great man to whom science owes its annual gatherings in Stockholm honoured me and my late friend, Prof. Nentsky,[28] with a letter enclosing a considerable gratuity for the best laboratory in our charge; in that letter Alfred Nobel displayed his keen interest in physiological experiments and suggested some of his own highly edifying schemes for experiments touching on the supreme tasks of physiology, the problems of the ageing and dying away of organisms. Indeed, physiology can justly anticipate big victories in this field; the power of physiology is by no means confined to what has already been achieved. This may be accomplished in the future only if our knowledge of the organism, which is an extremely com-

plex mechanism, is deepened and extended. An example in support of this has been just cited by me.

I shall now revert to the subject of my lecture. Among the stimuli of the digestive glands there is one category—it has not yet been mentioned—which, quite unexpectedly, came right into the foreground during our investigations. True, it has long been known that the sight of tasty food makes the mouth of a hungry man water; absence of appetite, too, has always been regarded as undesirable, from which it can be deduced that appetite is closely linked with the process of digestion. In physiology mention was made also of the psychical stimulation of both the salivary and gastric glands. It should be pointed out, however, that psychical stimulation of the gastric glands has not been universally recognized, and generally speaking, the outstanding role of psychical stimulation in the processing of food in the digestive canal has not met with proper acknowledgement. Our investigations forced us to bring these influences to the fore. Appetite, the craving for food, is a constant and powerful stimulus to the gastric glands. There is not a dog in which skilful teasing with food does not evoke a more or less considerable secretion of juice in the empty and hitherto inactive stomach. At the mere sight of food nervous and excitable animals secrete several hundred cubic centimetres of gastric juice, while the sedate and quiet animal secretes only a few cubic centimetres. By changing the experiment in a definite way, an extremely profuse secretion of juice is observed in all animals without exception; I have in mind the previously mentioned experiment with sham feeding, when the food cannot get from the mouth into the stomach. A very thorough and frequently repeated analysis of this experiment convinced us that in this case the secretion of juice cannot be regarded as being the result of a simple, reflex stimulation of the mouth and throat by the ingested food. Any chemical irritant can be introduced into the mouth of a dog operated upon in this way, and still the stimulation will not induce

the secretion of even a single drop of gastric juice. From this one might conclude that the oral cavity is stimulated not by every chemical substance but only by specific substances contained in the ingested food. However, continued observations did not confirm this supposition. The action of one and the same food, as a gland stimulus, differs, depending on whether the food was eaten by the animal with avidity or unwillingly, in response to command. Generally, the following invariable phenomenon is observed: each kind of food ingested by the dog during the experiment acts as a strong stimulus only when it suits the dog's taste. We must assume that in the act of eating the craving for food, that is, appetite—and therefore a psychical phenomenon—serves as a powerful and constant stimulus. The physiological significance of this juice, which we termed *appetite* juice, proved exceptionally great. If we introduce bread into the dog's stomach via a metal tube so as to prevent the dog from noticing it, i.e., without stimulating its appetite, the bread may remain in the stomach unchanged for a whole hour, without evoking even the slightest secretion of juice, since it lacks the substances that would stimulate the gastric glands. But when the same bread is swallowed by the animal, the gastric juice secreted in this case, that is, the appetite juice, exerts a chemical influence on the protein substances of the bread, or, in everyday terminology, digests it. Some of the substances obtained from protein subjected to this change act in turn on the gastric glands as independent stimuli; they thereby carry on the work begun by the first stimulus, the appetite, which is now, in the normal course of things, receding.

In the course of our study of the gastric glands we became convinced that appetite acts not only as a general stimulus to the glands, but that it also stimulates them in *varying degree*, depending on the object on which it is directed. For the salivary glands the rule obtains that all the variations of their activity observed in physiological experiments are exactly duplicated in the experiments with

psychical stimulation, i.e., in those experiments in which the given object is not brought into direct contact with the mucous membrane of the mouth, but attracts the animal's attention from a distance. For example, the sight of dry bread evokes a stronger secretion of saliva than the sight of meat, although the meat, judging by the animal's movements, may excite a much livelier interest. On teasing the dog with meat or any other edible substance a highly concentrated saliva flows from the submaxillary glands; on the contrary, the sight of disagreeable substances produces the secretion of a very fluid saliva from the same glands. In a word, the experiments with psychical stimulation prove to be exact, but miniature, models of the experiments with physiological stimulations by the same substances. Thus, with regard to the work of the salivary glands, psychology occupies a place close to that of physiology. More than that! At first sight the psychical aspect of this activity of the salivary glands appears even more incontrovertible than the physiological. When any object that attracts the attention of the dog from a distance produces salivary secretion, one has all the grounds for assuming that this is a psychical and not a physiological phenomenon. When, however, the dog has eaten something or substances have been forcibly introduced into its mouth, saliva begins to flow, it is still necessary to prove the presence in this phenomenon of a certain physiological cause, to demonstrate that it is not of a purely psychical character, but is reinforced because of the special conditions accompanying it. These concepts correspond all the more to reality, since, after the severance of all the sensory nerves of the tongue, most substances entering the mouth in the process of eating or forceful feeding, evoke, strange as it may seem, the identical pre-operative action of the salivary glands. It is necessary to go further and resort to more radical measures such as poisoning the animal or destroying the higher parts of the central nervous system, in order to become convinced that between substances stimulating the oral cavity and the salivary

glands there is not only a psychical but also a physiological connection. Thus we have two series of obviously different phenomena. But how is the physiologist to regard the psychical phenomena? It is impossible to disregard them because they are closely bound up with the purely physiological phenomena in the work of the digestive glands with which we are preoccupied. And if the physiologist intends to pursue his study of them he finds himself faced with the question: How?

Since we based ourselves on the experience acquired by us in the lowest organized representatives of the animal kingdom, and, naturally, desired to remain physiologists instead of becoming psychologists, we preferred to maintain a purely objective attitude also in regard to the psychical phenomena in our experiments with animals. Above all, we tried to discipline our thought and our speech in order completely to ignore the mental state of the animal; we limited our work to thorough observation and exact description of the influence exerted by distant objects on the secretion of the salivary glands. The results corresponded to our expectations—the relations between the external phenomena and the variations in the work of the glands could now be systematized; they proved to be of a regular character since they could be reproduced at will. To our great joy, we saw for ourselves that we had taken the right path in our observations, leading us to success. I shall cite some examples illustrating the results achieved by us with the help of these new methods.

If the dog is repeatedly teased with the sight of objects inducing a salivary secretion from a distance, the reaction of the salivary glands becomes weaker and weaker and finally drops to zero. The shorter the intervals between separate stimulations, the quicker the reaction reaches zero, and vice versa. These rules are fully manifested only when the conditions of the experiments remain unchanged. Identity of the conditions, however, may be only of a relative character; it may be confined only to those phenomena of the

external world that were previously associated with the act of eating or with the forceful introduction of corresponding substances into the animal's mouth; the change of other phenomena is of no significance. This identity is easily attained by the experimenter so that an experiment in which a stimulus repeatedly applied from a distance gradually loses its effect, can be readily demonstrated even in the course of one lecture. If in a repeated stimulation from a distance a certain substance becomes ineffective, this does not mean that the influence of other substances is thereby eliminated. For example, when milk ceases to stimulate the salivary glands, the action of bread remains strongly effective, and when the bread loses its effect owing to repetition of the experimental stimulation, acid or other substances still produce their full action on the glands. These relations also explain the real meaning of the above-mentioned identity of experimental conditions; every detail of the surrounding objects appears as a new stimulus. If a certain stimulus has lost its influence, it can be restored only after a rest of several hours duration. However, the lost action can be restored without fail at any time by special means.

If bread repeatedly shown to the dog no longer stimulates its salivary glands, it is only necessary to let the animal eat it and the effect of the bread placed at a distance is fully restored. The same result is obtained when the dog is given some other food. More than that. If a substance producing a salivary secretion, for instance, acid, is introduced into the dog's mouth, even then the original distant effect of bread is restored. Generally speaking, everything that stimulates the salivary glands restores the lost reaction; the greater their activity, the more fully it is restored.

However, the reaction can be inhibited with the same regularity by certain artificial means, if, for example, some extraordinary stimuli act on the eye or ear of the dog, evoking in the latter a strong motor reaction, say, a tremor of the whole body.

Since time is short I shall limit myself to what I have said and pass to a theoretical consideration of these experiments. Our facts fit in readily with physiological thought. The stimuli which act from a distance may be rightly termed and regarded as reflexes. Careful observation shows that the activity of the salivary glands is always excited by certain external phenomena, that like the usual physiological salivary reflex, it is caused by external stimuli. But while the latter emanates from the oral cavity, the former comes from the eye, nose, etc. The difference between the two reflexes is that our old physiological reflex is constant and unconditioned, whereas the new reflex is permanently subject to fluctuation, and is, therefore, *conditioned*. Examining the phenomena more closely we can see the following essential distinction between the two reflexes: in the unconditioned reflex the properties of the substance act as a stimulus with which the saliva has to deal physiologically, for example, the hardness, dryness, definite chemical properties, etc.; in the conditioned reflex, on the contrary, the properties of the substance which bear no direct relation to the physiological role of the saliva act as stimuli, for example, colour, etc. These last properties appear here as *signals* for the first ones. We cannot but notice in their stimulating action a wider and more delicate adaptation of the salivary glands to the phenomena of the external world. Here is an example. We are getting ready to introduce acid into the dog's mouth; in the interest of the integrity of the buccal mucous membrane it is obviously very desirable that before the acid enters the mouth, there should be more saliva; on the one hand the saliva hinders direct contact of the acid with the mucous membrane and, on the other hand, it immediately dilutes the acid, thus weakening its injurious chemical effect. However, in essence the signals have only a conditional significance: on the one hand, they are readily subject to change, and on the other, the signallizing object cannot come into contact with the mucous membrane of the mouth. Consequently, the finer

adaptation must consist in the fact that the properties of the signalling objects now stimulate the salivary glands, and at other times do not. And that is what really happens. Any phenomenon of the external world can be made a temporary signal of the object which stimulates the salivary glands, provided the stimulation of the mucous membrane of the mouth by the object has been associated once or more times with the action of the given external phenomenon on other receptor areas of the surface of the body. In our laboratory we are trying out many such highly paradoxical combinations; and the experiment is proving successful. On the other hand, rapidly acting signals can lose their stimulating effect if repeated over a long period without bringing the corresponding object into contact with the mucous membrane of the mouth. If ordinary food is shown to a dog for days and weeks, without giving it to him to eat, then the sight of the food will, finally, cease to produce a salivary secretion. The mechanism of stimulation of the salivary glands through the signalling properties of the objects, i.e., the mechanism of "conditioned stimulation," can be easily conceived from the physiological point of view as a function of the nervous system. As we have just seen, at the basis of each conditioned reflex, i.e., of stimulation through the signalling properties of an object, there is an unconditioned reflex, that is, a stimulation through the essential attributes of the object. Thus, it must be assumed that the point of the central nervous system which is strongly stimulated during the unconditioned reflex, attracts to itself weaker stimuli proceeding from the external world to other points of the central nervous system, i.e., thanks to the unconditioned reflex there is opened for all other external stimuli a temporary, casual path leading to the central point of this reflex. The conditions influencing the opening and closing of the path, its practicability and desolation, constitute the internal mechanism of the effectiveness or ineffectiveness of the signalling properties of the external objects; they are the physiological basis of the

most delicate reactivity of the living substance, of the most delicate adaptation of the animal organism.

It is my firm conviction that physiological research will be successfully and greatly advanced along the lines which I have sketched here.

In point of fact only one thing in life is of actual interest for us—our psychical experience. But its mechanism has been and still remains wrapped in mystery. All human resources—art, religion, literature, philosophy and historical science—have combined to throw light on this darkness. Man has at his disposal yet another powerful resource—natural science with its strictly objective methods. This science, as we all know, is making big headway every day. The facts and considerations which I have placed before you are one of the numerous attempts to employ—in studying the mechanism of the highest vital manifestations in the dog, the representative of the animal kingdom which is man's best friend—a *consistent*, purely scientific method of thinking.

IV

PROBLEM OF THE STUDY OF HIGHER NERVOUS ACTIVITY AND THE WAYS OF ITS EXPERIMENTAL SOLUTION

EXPERIMENTAL PSYCHOLOGY AND PSYCHOPATHOLOGY IN ANIMALS[29]

Regarding the language of facts as most eloquent, I shall take the liberty of proceeding directly to the experimental material, which gives me the right to speak on the subject of my present communication.

To begin with, this is the history of the transition of the physiologist from research into purely physiological problems to the sphere of phenomena usually called psychical. Although this transition took place suddenly, it occurred in a perfectly natural way, and what seems to me most important in this respect, without changing the, so to speak, methodological front.

In studying over a period of years the normal working of the digestive glands, and analysing the constant conditions of this work, I came upon conditions of a psychical character, which, incidentally, had been observed by others before me. There were no grounds for neglecting these conditions, since they participated constantly and prominently in the normal physiological process. I was obliged to investigate them if I wanted to make a really thorough study of my subject. But how? All that follows in my exposition supplies the answer to this question.

From all our material I shall select only the experiments with the salivary glands—organs which apparently play a very insignificant physiological role; however, I am convinced that they will become classical objects for the new type of research about which I shall have the honour of

telling you today; part of this research has already been carried out and part is in the planning stage.

In observing the normal working of the salivary glands one cannot but be amazed by the high degree of their adaptability.

Give the animal dry, hard food substances and there will be an abundant salivary secretion—give it liquid food and the secretion will be much smaller.

It is obvious that for the chemical testing of the food, for mixing it and converting it into a lump to be swallowed, water is required—and the salivary glands supply it. From the mucous salivary glands there flows for every kind of food, saliva rich in mucin—a lubricating saliva, which facilitates the smooth passage of the food into the stomach. All highly irritant substances, such as acids, salts, etc., also produce a salivary secretion which varies in accordance with the strength of their stimulating action; clearly, as we know from everyday experience, the purpose of this secretion is to neutralize or dilute the substances and to cleanse the mouth. In this case the mucous glands secrete fluid saliva containing little mucin. For what would be the purpose of the mucin here? If pure insoluble quartz pebbles are placed in the mouth of a dog it will move them around, try to chew them, and finally, it will drop them. There is either no secretion of saliva at all, or at most two or three drops flow out. Again, what purpose would the saliva serve here? The pebbles are easily ejected by the animal and nothing remains in the mouth. But if sand is placed in the dog's mouth, i.e., the same pebbles but in pulverized form, there will be an abundant flow of saliva. It is clear that without saliva, without fluid in the oral cavity, the sand could neither be ejected, nor forwarded to the stomach.

Here we have exact and constant facts—facts which seem to imply intelligence. But the entire mechanism of this intelligence is absolutely plain. On the one hand, physiology has long known about the centrifugal nerves of the salivary glands, which now chiefly cause water to enter into the sa-

liva, and now accumulate in the saliva special organic substances. On the other hand, the internal lining of the oral cavity consists of separate areas which act as receptors of different special stimuli—mechanical, chemical, thermal. Moreover, these stimuli may be further subdivided, the chemical, for example, into salts, acids, etc. There are grounds for assuming that the same thing is true of the mechanical stimuli. It is in the areas acting as receptors of special stimuli that the specific centripetal nerves have their origin.

Thus, the reactions of adaptation are based on a simple reflex originated by definite external conditions acting only on certain kinds of centripetal nerve endings; from here the excitation passes along a definite nervous path to the centre, and thence, also along a definite path, to the salivary gland, evoking its specific function.

In other words, this is a specific external agent evoking a specific reaction in living matter. At the same time we have here a typical example of what we call adaptation or fitness. Let us dwell for a moment on these facts and terms, since they play, obviously, an important role in modern physiological thought. What, exactly, is the fact of adaptation? It is, as we have just seen, simply the exact co-ordination of the elements making up a complex system and of the entire complex with the surrounding world.

But the same thing can be observed in any inanimate object. Take, for example, a complex chemical object. This object exists thanks to equilibration between its separate atoms and groups, between the object as a whole and the surroundings.

In exactly the same way the immense complexity of the higher and lower organisms exists as a whole so long as all its constituents are delicately and strictly co-ordinated and equilibrated both with one another and with the external conditions.

The analysis of the equilibration of this system is the prime task and aim of physiological investigation as purely objective investigation. There can hardly be two opinions

on this point. Unfortunately, so far we have no purely scientific term to denote this fundamental property of the organism, its external and internal equilibrium. Many people hold that the terms now in use—fitness and adaptation (despite their natural-scientific, Darwinist analysis)—bear the stamp of subjectivism, which leads to misunderstanding in two opposite directions. The rigid adherents of the physico-mechanical theory of life see in these words an anti-scientific tendency—a retreat from pure objectivism to speculation and teleology.[30] On the other hand, philosophically inclined biologists see in every fact relating to adaptation and fitness proof of the existence of a special vital force, or, as it is now more and more often called, spiritual force (vitalism, apparently, gives way to animism[31]), which defines its own goal, chooses its means, adapts itself, etc.

And so, in the afore-mentioned physiological experiments with the salivary glands we, in our investigation, remain strictly within the bounds of natural science. We shall now pass to another sphere of phenomena which, it would seem, belong to quite a different category.

All the foregoing objects, which, after being placed in the mouth, influenced the salivary glands in different and at the same time definite ways, exert on these glands exactly the same action, at least qualitatively, when placed at a certain distance from the dog. Dry food produces much saliva—moist food only a little. A thick, lubricating saliva flows from the mucous glands to the food substances. Various inedible irritants also produce secretion from all the glands, including the mucous glands. But it is fluid and contains but a small amount of mucin. Pebbles, when shown to the animal, have no effect on the glands, while sand evokes profuse salivation. These facts were partly obtained and partly systematized in my laboratory by Dr. S. G. Wolfson. The dog sees, hears, and smells all these substances, pays attention to them, rushes to them if edible or agreeable, but turns away from them and resists their introduction into the mouth when disagreeable. Everybody would say that this is

a psychical reaction, psychical stimulation of the animal's salivary glands.

How should the physiologist regard these facts? How can he establish them? How to analyse them? What are they compared with physiological facts? What are their common features and in what way are they distinguished from one another?

Must we, for the purpose of getting to know the new phenomena, penetrate into the inner state of the animal, visualize its feelings and desires in our own way?

It seems to me that for the naturalist there is only one answer to the last question—an emphatic "No." Where is there even the slightest indisputable criterion that our conjectures are correct, that we can, for the sake of a better understanding of the matter, compare the inner state of even such a highly developed animal as the dog with our own? Further: is not the eternal sorrow of life the fact that in most cases human beings do not understand each other and cannot enter into the inner state of the other? And then, where is the knowledge, where is the power of knowledge that might enable us correctly to comprehend the state of another human being? At first, in our psychical experiments with the salivary glands (for the time being we shall use the term "psychical"), we conscientiously endeavoured to explain our results by imagining the subjective state of the animal. But nothing came of this except sterile controversy and individual views that could not be reconciled. And so we could do nothing but conduct the research on a purely objective basis; our first and especially important task was completely to abandon the very natural tendency to transfer our own subjective state to the mechanism of the reaction of the animal undergoing the experiment and to concentrate instead on studying the correlation between the external phenomena and the reaction of the organism, i.e., the activity of the salivary glands. Reality had to decide whether elaboration of the new phenomena was possible in that direction. I make bold to say that the following account will

convince you, as I am convinced, that a boundless field of fruitful research opens before us in the given case; it is another and immense part of the physiology of the nervous system, a system which mainly establishes the correlation not between the separate parts of the organism, our main subject so far, but between the organism and the surroundings. Unfortunately, to date the influence of the surrounding world on the nervous system has been studied mainly in relation to subjective reactions—the content of the modern physiology of the sense organs.

In our psychical experiments we have before us definite external objects, exciting the animal and evoking in it a definite reaction, in the given case—secretion of the salivary glands. As has been said, the effect of these objects is substantially the same as in the physiological experiments, when they come into contact with the oral cavity. Consequently, we have before us simply further adaptation—the object acts on the salivary glands the moment it is being brought close to the mouth.

What are the specific features of these new phenomena compared with the physiological ones? Above all, the difference seems to be that in the physiological form of the experiment the substance comes into direct contact with the organism, while in the psychical form it acts from a distance. But this circumstance in itself, if we reflect on it, does not, obviously, signify any essential difference between these, in a way specific, experiments, and the purely physiological ones. The point is that in these cases the substances act on other special receiving surfaces of the body—nose, eye, ear—through the medium in which both the organism and the stimulating substances exist (air, ether). How many simple physiological reflexes are transmitted by the nose, eye and ear, that is, originate at a distance! Hence, the essential difference between the new phenomena and the purely physiological does not lie here.

It lies much deeper, and should be sought, in my view, in a comparison of the following facts. In the physiological

case the activity of the salivary glands is connected with the properties of the substance on which the effect of the saliva is directed. The saliva moistens dry substances and any ingested material; it neutralizes the chemical effect of the substances. These properties constitute the special stimuli of the specific mouth surface. Consequently, in the physiological experiments the animal is stimulated by the essential, unconditioned properties of the object in relation to the physiological role of the saliva.

In the psychical experiments the animal is excited by the properties of the external object, which are unessential for the activity of the salivary glands, or even entirely accidental. The visual, acoustic and even purely olfactory properties of our objects, when they are present in other objects, do not of themselves exert any influence on the salivary glands which, in their turn, so to speak, have no business relations with these properties. In the psychical experiments the salivary glands are stimulated not only by the properties of the objects unessential for the work of the glands, but absolutely by all the conditions surrounding these objects, or with which they are connected one way or another—for example, the dish in which they are contained, the article on which they are placed, the room, the people who usually bring the objects, even the noises produced by these people, though the latter may not be seen at the given moment—their voices, even the sound of their steps. Thus, in psychical experiments, the connection of the objects acting as stimuli on the salivary glands becomes more and more distant and delicate. Here, undoubtedly, we have a phenomenon of further adaptation. We can admit in this case that such a distant and delicate connection as that between the step of the person who usually feeds the animal and the working of the salivary glands has no specific physiological significance other than its delicacy. But we need only recall those animals whose saliva contains protective poison, to appreciate the great vital significance of this timely provi-

sion of a protective means against an approaching enemy. The significance of the distant signs of objects producing a motor reaction in the organism, is, of course, easily recognized. By means of distant and even accidental characteristics of objects the animal seeks its food, evades enemies, etc.

If that is so, then the following questions are of decisive significance for our subject: can this seemingly chaos of relations be included in a definite scheme? Is it possible to make the phenomena constant, to disclose the laws governing their development and their mechanism? It seems to me that the examples which I shall now present entitle me to give an emphatically positive answer to these questions, to find at the basis of all psychical experiments the one and same special reflex as the chief and most general mechanism. True, in its physiological form, our experiment, excluding, of course, all extraordinary conditions, always yields one and the same result; it is the unconditioned reflex. But the main feature of the psychical experiment is its impermanence, its obvious capriciousness. However, the results of a psychical experiment undoubtedly recur too, otherwise we would not speak of them at all. Consequently, the point is in the greater number of factors which influence the results of a psychical experiment compared with a physiological one. This, then, is a conditioned reflex. Here are facts which show that our psychical material may also be included in a definite scheme and that it is subject to certain laws. These facts were obtained in my laboratory by Dr. I. F. Tolochinov.

It is not difficult to recognize during the first psychical experiments the chief conditions guaranteeing their success, i.e., their constancy. If an animal is stimulated (i.e., its salivary glands) by food placed at a distance, the result of the experiment depends solely on whether the animal has been prepared for it by a certain period of fasting. An animal experiencing keen hunger yields positive results; on the contrary, the most voracious and least fastidious animal, if it

has just had a good meal, fails to respond to food placed at a distance. Thinking in terms of physiology we can say that we have here a different degree of excitability of the salivary centre—greatly increased in the first case, and greatly decreased in the second. We may rightly assume that just as the carbonic acid contained in the blood determines the energy of the respiratory centre, so the different composition of the blood in a hungry animal and in one that is sated determines the above-mentioned fluctuations in the excitability and reactivity of the salivary centres. From the subjective point of view this could be designated as attention. When the stomach is empty, the sight of food easily causes the mouth to water; in sated animals the same reaction is either very weak or entirely lacking.

Let us proceed. If the animal is shown food or certain disagreeable substances, and if this is repeated several times, then with each repetition the experiment will produce a weaker result, and in the end there will be no reaction whatever. There is, however, a sure method of restoring the reaction; it can be achieved by giving the dog food or by introducing into its mouth substances which ceased to act as stimuli. This, of course, produces the usual strong reflex, and the object begins to act from a distance again. For the subsequent result it is immaterial whether food is placed in the mouth or any disagreeable substance. For example, if meat powder no longer stimulates the animal from a distance, its effect can be restored either by letting it eat the powder or by introducing into the mouth an undesired substance, e.g., acid. We can say that thanks to the direct reflex the excitability of the salivary centre has been heightened, and the weak stimulus—the object at a distance—has become sufficiently strong. Do we not experience the same thing ourselves when appetite comes with eating, or when, after unpleasant, powerful excitation, we begin to have the appetite that we previously lacked?

Here is a number of other facts of a constant character. The object placed at a distance stimulates the salivary

glands not only by the entire complex of its properties, but also by its individual properties. If a hand smelling of meat or meat powder, is brought into proximity with the dog, it often proves sufficient to induce a salivary reaction. Similarly the sight of food placed at a distance, and consequently the mere optical effect of the object, may also stimulate the activity of the salivary glands. But the combined, simultaneous action of all these properties of the object always produces a better and greater effect, i.e., the action of the sum of the stimuli is more powerful than each individual stimulus.

The object acts on the salivary glands from a distance not only by means of its inherent properties but also by means of incidental qualities deliberately imparted to it. If we colour the acid black, then even water to which the same colour is added will influence the salivary glands from a distance. However, all incidental qualities deliberately imparted to the distant object begin to act as stimuli of the salivary glands only when the object with its newly acquired properties is brought into contact with the oral cavity at least once. Black-coloured water began to stimulate the salivary glands from a distance only after preliminary introduction of black-coloured acid into the dog's mouth. The stimuli of the olfactory nerves belong to the same group of conditioned properties. The experiments carried out in our laboratory by Dr. A. T. Snarsky showed that simple physiological reflexes from the nasal cavity acting on the salivary glands are conducted only through the sensory nerves lying along the trigeminal nerve. Ammonia, mustard oil, etc., always produce an invariable effect even in a curarized animal. However, if the trigeminal nerves are severed, this action fails. Odours lacking a local stimulating effect have no influence on the salivary glands. If, for example, oil of anise is placed for the first time before a normal dog with constant salivary fistulae there will be no secretion of saliva. But if simultaneously with the odour the oil of anise (which produces a strong local irritation) is brought into contact

with the dog's oral cavity, saliva secretion will be induced afterwards by the odour alone.

If food is combined with a disagreeable substance or with a certain property of the disagreeable substance, for example, if you show the dog meat moistened with acid, then, despite the fact that the dog reaches for the meat, a saliva secretion comes from the parotid gland (for meat alone there is no secretion from this gland), i.e., a reaction to the disagreeable substance. Moreover, if owing to repetition the action of the disagreeable substance placed at a distance becomes insignificant, then upon combining it with food which attracts the animal, the reaction always becomes intensified.

As mentioned above, dry food causes abundant saliva secretion, while moist food, on the contrary, produces either a weak flow of saliva or none at all. If one acts on a dog from a distance by showing it two extremes, for instance, dry bread and moist meat, the result will depend on the object which stimulates the dog more strongly, and this can be judged by its motor reaction. If, as usually happens, the dog is stimulated more strongly by the meat, then the only reaction will be the one peculiar to meat, i.e., there will be no saliva secretion. Thus, the bread, although it is before the dog's eyes, remains ineffective. It is possible to impart the smell of sausage or meat to dry bread so that the bread alone acts on the dog's eye, with the sausage or meat only leaving a smell, and yet the only reaction will be that induced by the sausage or meat.

The action of objects from a distance can be inhibited in other ways. If in the presence of a greedy, excitable dog we feed another dog, for example with dry bread, then the salivary glands, which previously evinced a most vivid reaction to the sight of bread, become inactive.

When the dog is placed on the stand for the first time, the sight of dry bread, which produced a very strong action on the salivary glands when the dog was on the floor, now has not even the slightest influence.

I have placed before you a number of easily and exactly recurring facts. It will be obvious that many striking instances of animal training belong to the same category as some of our facts. It follows, therefore, that they have long since testified to the strictly law-governed nature of certain psychical manifestations in animals. It is to be regretted that they have been left so long without science giving them the attention they merit.

So far in my exposition I have not mentioned any phenomenon corresponding to what in the subjective world we call desires. Actually we have not encountered such phenomena. On the contrary, the following fundamental fact constantly recurred before our eyes: the sight of dry bread, to which the dog hardly turned its head, produced an abundant secretion of saliva, whereas meat, to which the dog rushed with avidity, breaking from the stand and gnashing its teeth, failed to exert any influence on the salivary glands when placed at a distance. Thus, what in the subjective world we designate a desire, was expressed in our experiments only by the animal's motor reaction, but did not manifest any positive action on the salivary glands. Hence, the phrase that ardent desire stimulates the salivary or gastric glands in no way corresponds to reality. This sin of confusing what are obviously different things can be imputed also to me in my earlier articles. In our experiments we must, therefore, clearly distinguish between the secretory and the motor reactions of the organism; and in respect to the glands, if we compare our results with the phenomena of the subjective world, we must regard not the desire of the dog, but its attention, as the chief condition for the success of the experiments. The salivary reaction of the animal might be regarded in the subjective world as a substratum of elementary, pure notion, thought.

The above-mentioned facts, on the one hand, provide certain, and in my view, important conclusions about the processes taking place in the central nervous system; on the other hand, they make possible further successful analysis.

Let us consider from the standpoint of physiology some of our facts, and first of all our fundamental fact. When a given object—a certain food or chemical irritant—is brought into contact with the special surface of the oral cavity and stimulates it by means of those of its properties on which the activity of the salivary glands is specially directed, then the other properties of the object that have nothing to do with the working of the salivary glands or even with the entire environment of the object, but simultaneously stimulating other sensory surfaces of the body, become connected, apparently, with the same nervous centre of the salivary glands to which the stimulation emanating from the essential properties of the object is conducted through a fixed centripetal path. It can be assumed in this case that the salivary centre acts in the central nervous system as a point of attraction for stimuli coming from other sensory surfaces. Thus, a certain path is opened from the other excited areas of the body to the salivary centre. But this connection of the centre with accidental points is very fragile and tends to disappear of itself. Constant repetition of simultaneous stimulation by means of the essential and unessential properties of the object is required to make this connection increasingly durable. In this way a temporary relation is established between the activity of a certain organ and the external objects. The temporary relation and its law—to become stronger as a result of repetition and to disappear when not repeated—play a big role in the well-being and integrity of the organism; by means of it the adaptability of the organism and the conformity of its activity to the surroundings become more perfect and delicate. The two parts of this law are equally important: if the temporary relation to the object is of great significance for the organism, then the rupture of this relation is essential when it is no longer justified by reality. Otherwise the relations of the animal, instead of being delicate, would assume a chaotic character.

Let us turn to another point. From the standpoint of physiology how do we regard the fact that the sight of meat

destroys in the parotid gland the reaction to the sight of bread, i.e., that the saliva earlier secreted at the sight of bread ceases to flow when there is a simultaneous meat stimulation? One could assume that strong excitation in a definite motor centre corresponds to the strong motor reaction provided by the meat, as a consequence of which, according to the above-mentioned law, the stimulation is diverted from other parts of the central nervous system and in particular from the salivary centres, i.e., their excitability is diminished. This interpretation is supported by another experiment in which the secretion of saliva at the sight of bread is inhibited by the sight of another dog. Here the motor reaction to bread is really greatly intensified. Even more convincing would be an experiment in which the dog would prefer dry food to moist and display a stronger motor reaction to it. We should be quite right in our interpretation of this experiment if in the dog in question the sight of dry food did not evoke secretion of saliva, or if the secretion should be much less than in usual dogs. It is a well-known fact that a very strong desire often inhibits certain special reflexes.

But among the above-mentioned facts there are some which, from the physiological point of view, can be explained only with great difficulty; for example, why does a conditioned reflex, when repeated, invariably become ineffective? The natural explanation of fatigue is hardly acceptable, since in this case we are dealing with a weak stimulus. Actually the repetition of a strong stimulus of an unconditioned reflex does not bring on early fatigue. Probably we have here altogether peculiar conditions for the excitation which is conducted along accidental centripetal paths.

From the above it is obvious that our new subject can be investigated quite objectively and that, in essence, it is a purely physiological subject. One can hardly doubt that analysis of this group of stimuli, coming into the nervous system from the external world will reveal to us laws of

nervous activity and disclose its mechanism from aspects which so far have not been even touched upon by investigation of nervous phenomena in the organism, or have been touched upon only in rough outline.

Despite the complexity of the new phenomena, this investigation entails considerable advantages. In the present study of the mechanism of the nervous system, first, the experiments are conducted on animals that have just been injured by operations, and, secondly, and this is the chief thing, the nerve trunks of the animals are subjected to stimulation, i.e., the excitation extends simultaneously and in a uniform manner over a mass of highly diverse nervous fibres; such combinations, however, never occur in reality. Naturally, we experience great difficulty in discovering the laws of the normal activity of the nervous system, since we bring it to a state of chaos by our artificial stimulation. But in normal conditions, such as we maintained in our latest experiments, the stimulation is effected in an isolated manner, the correlations of the intensity being regulated.

Generally speaking this applies to all psychical experiments, but in the case of our psychical phenomena, observed in the activity of the salivary glands, there is another special advantage. For successful investigation of a subject, complex by its very nature, it is important to simplify it in some way or other. In the given case we have this simplification. The role of the salivary glands is so clear that their relation to the external environment of the organism must be equally clear and accessible to investigation and interpretation. However, it must not be imagined that the physiological role of the salivary glands is confined to the abovementioned functions. By no means. For example, saliva is used by the animals for licking and healing wounds, a thing that we constantly see. This is probably the reason why we can obtain saliva by stimulating various sensory nerves. And yet the complexity of the physiological relations of the salivary glands is much less than that of the skeletal muscles through which the organism is connected with the ex-

ternal world in an endless number of ways. At the same time a simultaneous comparison of the secretory, especially salivary, reaction with the motor reaction enables us, on the one hand, to distinguish between the particular and the general, and on the other hand, to get rid of our stock of routine anthropomorphic concepts and interpretations relating to the motor reaction of the animals.

Having established the possibility of analysis and systematization of our phenomena we come to the next stage of our work—systematic division and derangement of the central nervous system in order to see how the previously established relations change. Thus, there will be an anatomical analysis of the mechanism of these relations. This will be the future and, I feel sure, the already approaching experimental psychopathology.

Here, too, the salivary glands as objects of investigation are of great value. The nervous system, which has a bearing on movement, is so highly intricate and predominates to such an extent in the brain that even the slightest damage to it often causes undesirable and very complicated results. The nervous system of the salivary glands, because of their inconsiderable physiological significance, comprises, it may be assumed, only a negligible portion of the brain substance, and is, consequently, so thinly distributed in the brain that its partial, isolated destruction does not bring about, even remotely, the difficulties which in this respect exist in the innervation motor apparatus. Of course, psychopathological experiments had their beginning at a time when the physiologists first removed these or other parts of the central nervous system and investigated the animals that survived these operations. In this respect the past twenty or thirty years have supplied us with a number of fundamental facts. We already know the drastic decline that takes place in the adaptive capacity of animals as a result of complete or partial extirpation of their cerebral hemispheres. But the investigation of this subject has not yet developed into a special branch, which could be studied without interruption and

according to a definite plan. The reason for this, as I see it, is that the investigators still lack the more or less considerable and detailed knowledge of the animal's normal relations with the surrounding world that would enable them to make an objective and exact comparison of the state of the animal before and after the operation.

Objective investigation alone will gradually bring us to the complete analysis of that infinite adaptability in all its manifestations which constitutes life on earth. Are not the movements of plants towards light and the seeking of truth through mathematical analysis essentially phenomena of one and the same order? Are they not the last links of an almost endless chain of adaptation taking place throughout the living world?

We can analyse adaptation in its most elementary forms on the basis of objective facts. Is there any reason for changing this method in the study of adaptability in the higher orders?

Work in this direction has been started at different levels of life and has advanced without encountering obstacles. The objective study of living matter, which begins with the theory of tropisms of elementary living things, can and must remain objective also when it reaches the highest manifestations of the animal organism, the so-called psychical phenomena in the higher animals.

Guided by the similarity or identity of external manifestations, science, sooner or later, will apply the objective facts also to our subjective world and thereby shed a bright light on our mysterious nature, elucidate the mechanism and the vital significance of that which occupies the human mind most—his consciousness and its torments. This explains why in my exposition I have some words which sounded as if they were contradictory. In the title of my paper and throughout my exposition I have used the term "psychical," at the same time bringing forward only objective investigation and leaving aside everything subjective. The vital phenomena that are termed psychical, despite the fact that they

are objectively observed in animals, are only distinguished from purely physiological phenomena by degree of complexity. It makes no difference whether they are termed psychical or complex-nervous as distinct from the simple physiological, once it is realized and recognized that the naturalist should approach them only objectively, leaving aside the question of the essence of these phenomena.

Is it not clear that contemporary vitalism, that is, animism, confuses the different points of view of the naturalist and of the philosopher. The former always bases his grandiose success on the study of objective facts and their comparisons, disregarding on principle the question of the essence and final causes; the latter, personifying the highest aspiration of man for synthesis—although up to now it has been of a fantastic nature—and seeking to provide the answer to everything relating to the human being, must right now create an entity from the objective and the subjective. For the naturalist everything lies in the method, in the chance of obtaining an unshakeable, lasting truth; and solely from this point of view, which for him is obligatory, the soul, as a naturalistic principle, is not only unnecessary but even harmful to his work, in vain limiting his courage and the depth of his analysis.

V

METHODS OF INVESTIGATION AND FUNDAMENTAL LAWS OF DEVELOPMENT

LECTURES ON THE WORK OF THE CEREBRAL HEMISPHERES[32]

LECTURE ONE

The substantiation and the history of the fundamental methods employed in the investigation of the activity of the cerebral hemispheres. The concept of the reflex. The variety of reflexes. Signalling activity as the most general physiological characteristic of the cerebral hemispheres.

Gentlemen,

One cannot but be struck by a comparison of the following facts. First, the cerebral hemispheres, the higher part of the central nervous system, is a rather impressive organ. In structure it is exceedingly complex, comprising millions and millions (in man—even billions) of cells, i.e., centres or foci of nervous activity. These cells vary in size, shape and arrangement and are connected with each other by countless branches. Such structural complexity naturally suggests a very high degree of functional complexity. Consequently, it would seem that a boundless field of investigation is offered here for the physiologist. Secondly, take the dog, man's companion and friend since prehistoric times, in its various roles as hunter, sentinel, etc. We know that this complex behaviour of the dog, its higher nervous activity (since no one will dispute that this is higher nervous activity), is chiefly associated with the cerebral hemispheres. If we remove the cerebral hemispheres in the dog (Goltz and others), it becomes incapable of performing not only the roles mentioned above, but even of looking after itself. It

becomes profoundly disabled and will die unless well cared for. This implies that both in respect of structure and function, the cerebral hemispheres perform considerable physiological work.

Let us turn now to man. His entire higher nervous activity is also dependent on the normal structure and functioning of the cerebral hemispheres. The moment the complex structure of his hemispheres is damaged or disturbed in one way or another, he also becomes an invalid; he can no longer freely associate with his fellows as an equal and must be isolated.

In amazing contrast to this boundless activity of the cerebral hemispheres is the scant content of the present-day physiology of these hemispheres. Up to 1870 there was no physiology of the cerebral hemispheres at all; they seemed inaccessible to the physiologist. It was in that year that Fritsch and Hitzig first successfully applied the ordinary physiological methods of stimulation and destruction to their study. Stimulation of certain parts of the cerebral cortex regularly evoked contractions in definite groups of the skeletal muscles (the cortical motor region). Extirpation of these parts led to certain disturbances in the normal activity of the corresponding groups of muscles.

Shortly afterwards H. Munk, Ferrier and others demonstrated that other regions of the cortex, seemingly not susceptible to artificial stimulation, are also functionally differentiated. Removal of these parts leads to defects in the activity of certain receptor organs—the eye, the ear and the skin.[33]

Many researchers have been thoroughly investigating these phenomena. More precision and more details have been obtained, especially as regards the motor region, and this knowledge has even found practical application in medicine; however, investigation as yet has not gone far beyond the initial point. The essential fact is that the entire higher and complex behaviour of the animal, which is dependent on the cerebral hemispheres, as shown by the pre-

viously mentioned experiment by Goltz with the extirpation of the hemispheres in a dog, has hardly been touched upon in these investigations and is not included even in the programme of current physiological research. What do the facts relating to the cerebral hemispheres, which are now at the disposal of the physiologist, explain with regard to the behaviour of the higher animals? Is there a general scheme of the higher nervous activity? What kind of general rules govern this activity? The contemporary physiologist finds himself truly empty-handed when he has to answer these lawful questions. While the object of investigation is highly complex in relation to structure, and extremely rich in function, research in this sphere remains, as it were, in a blind alley, unable to open up before the physiologist the boundless vistas which might have been expected.

Why is this so? The reason is clear: the work of the cerebral hemispheres has never been regarded from the same point of view as that of other organs of the body, or even other parts of the central nervous system. It has been described as special *psychical* activity which we feel and apprehend in ourselves and which we suppose exists in animals by analogy with human beings. Hence the highly peculiar and difficult position of the physiologist. On the one hand, the study of the cerebral hemispheres, as of all other parts of the organism, seems to come within the scope of physiology, but on the other hand, it is an object of study by a special branch of science—psychology. What, then, should be the attitude of the physiologist? Should he first acquire psychological methods and knowledge and only then begin to study the activity of the cerebral hemispheres? But there is a real complication here. It is quite natural that physiology, in analysing living matter, should always base itself on the more exact and advanced sciences—mechanics, physics and chemistry. But here we are dealing with an altogether different matter, since in this particular case we should have to rely on a science which has no claim to exactness as compared with physiology. Until recently discus-

sion revolved even around the question whether psychology should be considered a natural science or a science at all. Without going deeply into this question, I should like to cite some facts which, although crude and superficial, seem to me very convincing. Even the psychologists themselves do not regard their science as being exact. Not so long ago James, an outstanding American psychologist,[34] called psychology not a science, but a "hope for science." Another striking illustration has been provided by Wundt,[35] formerly a physiologist, who became a celebrated psychologist and philosopher and even the founder of the so-called experimental psychology. Prior to the war, in 1913, a discussion took place in Germany as to the advisability of separating the psychological branch of science from the philosophical in the universities, i.e., of having two separate chairs instead of one. Wundt opposed separation, one of his arguments being the impossibility of establishing a common and obligatory examination programme in psychology, since each professor had his own ideas of the essence of psychology. Is it not clear, then, that psychology has not yet reached the stage of an exact science?

This being the case, there is no need for the physiologist to have recourse to psychology. In view of the steadily developing natural science it would be more logical to expect that not psychology should render assistance to the physiology of the cerebral hemispheres, but, on the contrary, physiological investigation of the activity of this organ in animals should lay the foundation for the exact scientific analysis of the human subjective world. Consequently, physiology must follow its own path—the path blazed for it long ago. Taking as his starting-point the assumption that the functioning of the animal's organism, unlike that of the human being, is similar to the work of a machine, Descartes[36] three hundred years ago evolved the idea of the reflex as the basic activity of the nervous system. Descartes regarded every activity of the organism as a natural response to certain external agents and believed that the connection

between the active organ and the given agent, that is, between cause and effect, is achieved through a definite nervous path. In this way the study of the activity of the animal nervous system was placed on the firm basis of natural science. In the eighteenth, nineteenth and twentieth centuries the idea of the reflex had been extensively used by physiologists, but only in their work on the lower parts of the central nervous system; gradually, however, they began to study its higher parts, until finally, after Sherrington's[37] classical works on spinal reflexes, Magnus,[38] his successor, established the reflex nature of all the basic locomotor activities of the organism. And so experiment fully justified the idea of the reflex which, thereafter, was used in the study of the central nervous system almost up to the cerebral hemispheres. It is to be hoped that the more complex activities of the organism, including the basic locomotor reflexes —states so far referred to in psychology as anger, fear, playfulness, etc.—will soon be related to the simple reflex activity of the subcortical parts of the brain.

A bold attempt to apply the idea of the reflex to the cerebral hemispheres not only of animals but also of man, was made by I. M. Sechenov, the Russian physiologist, on the basis of the contemporary physiology of the nervous system. In a paper published in Russian in 1863 and entitled *Reflexes of the Brain* Sechenov characterized the activity of the cerebral hemispheres as reflex, i.e., determined activity. He regarded thoughts as reflexes in which the effector end is inhibited, and affects as exaggerated reflexes with a wide irradiation of excitation. A like attempt has been made in our time by Ch. Richet who introduced the concept of the psychical reflex in which the reaction to a given stimulus is determined by its union with the traces left in the cerebral hemispheres by previous stimuli. Generally, the recent physiology of the higher nervous activity related to the cerebral hemispheres tends to associate acting stimulations with traces left by previous ones (associative memory—according to J. Loeb; training, education by experience—ac-

cording to other physiologists). But this was mere theorizing. The time had come for a transition to the experimental analysis of the subject, and from the objective external aspect, as is the case with any other branch of natural science. This transition was determined by comparative physiology[39] which had just made its appearance as a result of the influence of the theory of evolution. Now that it had turned its attention to the entire animal kingdom, physiology, in dealing with its lower representatives, was forced, of necessity, to abandon the anthropomorphic concept and concentrate on the scientific elucidation of the relations between the external agents influencing the animal and the responsive external activity, the locomotor reaction of the latter. This gave birth to J. Loeb's doctrine of animal tropisms;[40] to the suggestion by Beer, Bethe and Uexküll of an objective terminology for designating the animal reactions; and finally, to the investigation by zoologists of the behaviour of the lower representatives of the animal world, by means of purely objective methods, by comparing the effect of external influences on the animal with its responsive external activity—as for example in the classical work of Jennings,[41] etc.

Influenced by this new tendency in biology and having a practical cast of mind, American psychologists who also became interested in comparative psychology displayed a tendency to subject the external activity of animals to experimental analysis under deliberately induced conditions. Thorndike's[42] *Animal Intelligence* (1898) must be regarded as the starting-point for investigations of this kind. In these investigations the animal was kept in a box and food placed outside, within sight. The animal, naturally, tried to reach the food, but to do so it had to open the door which in the different experiments was fastened in a different way. Tables and charts registered the speed and the manner in which the animal solved this problem. The entire process was interpreted as the formation of an association, connection between the visual and the tactile stimulation and the locomo-

tor activity. Afterwards by means of this method, and by modifications of it, researchers studied numerous questions relating to the associative ability of various animals. Almost simultaneously with the above-mentioned work by Thorndike, of which I was not then aware, I too had arrived at the idea of the need for a similar attitude to the subject. The following episode, which occurred in my laboratory, gave birth to the idea.

While making a detailed investigation of the digestive glands I had to busy myself also with the so-called psychical stimulation of the glands. When, together with one of my collaborators, I attempted a deeper analysis of this fact, at first in the generally accepted way, i.e., psychologically, visualizing the probable thoughts and feelings of the animal, I stumbled on a fact unusual in laboratory practice. I found myself unable to agree with my colleague; each of us stuck to his point of view, and we were unable to convince each other by certain experiments.[13] This made me definitely reject any further psychological discussion of the subject, and I decided to investigate it in a purely objective way, externally, i.e., strictly recording all stimuli reaching the animal at the given moment and observing its corresponding responses either in the form of movements or in the form of salivation (as occurred in this particular case).

This was the beginning of the investigations that I have carried on now for the past twenty-five years with the participation of numerous colleagues who joined hand and brain with me in this work and to whom I am deeply grateful. We have, of course, passed through different stages, and the subject has been advanced only gradually. At first we had but a few separate facts at our disposal, but today so much material has been accumulated by us that we can make an attempt to present it in a more or less systematized form. I am now in a position to place before you a physiological theory of the activity of the cerebral hemispheres which at any rate conforms much more to the structural and functional complexity of this organ than the theory which until

now has been based on a few fragmentary, though very important, facts of modern physiology.

Thus, research along these new lines of strictly objective investigation of the higher nervous activity has been carried out mainly in my laboratories (with the participation of a hundred colleagues); work along the same lines has been carried out also by American psychologists. As for other physiological laboratories, so far only a few have begun, starting somewhat later, to investigate this subject, but in most cases their work is still in the initial stage. So far there has been one essential point of difference in the research of the Americans and in ours. Since in the case of the Americans the objective investigation is being conducted by psychologists, this means that, although psychologists study the facts from the purely external aspect, nevertheless, in posing the problems, in analysing and formulating the results, they tend to think more in terms of psychology. The result is that with the exception of the group of "behaviourists"[44] their work does not bear a purely physiological character. Whereas, we, having started from physiology, invariably and strictly adhere to the physiological point of view, and we are investigating and systematizing the whole subject solely in a physiological way.

I shall now pass to an exposition of our material, but before doing so I should like to touch on the concept of the reflex in general, on reflexes in physiology and the so-called instincts.

In the main we base ourselves on Descartes' concept of the reflex. Of course, this is a genuinely scientific concept, since the phenomenon implied by it can be strictly determined. It means that a certain agent of the external world, or of the organism's internal medium produces a certain effect in one or other nervous receptor, which is transformed into a nervous process, into nervous excitation. The excitation is transmitted along certain nerve fibres, as if along an electric cable, to the central nervous system; thence, thanks

to the established nervous connections, it passes along other nerve fibres to the working organ, where it in its turn is transformed into a special activity of the cells of this organ. Thus, the stimulating agent proves to be indispensably connected with the definite activity of the organism, as cause and effect.

It is quite obvious that the entire activity of the organism is governed by definite laws. If the animal were not (in the biological sense) strictly adapted to the surrounding world, it would, sooner or later, cease to exist. If instead of being attracted by food, the animal turned away from it, or instead of avoiding fire threw itself into it, and so on, it would perish. The animal *must* so react to the environment that all its responsive activity ensures its existence. The same is true if we think of life in terms of mechanics, physics and chemistry. Every material system can exist as an entity only so long as its internal forces of attraction, cohesion, etc., are equilibrated with the external forces influencing it. This applies in equal measure to such a simple object as a stone and to the most complex chemical substance, and it also holds good for the organism. As a definite material system complete in itself, the organism can exist only so long as it is in equilibrium with the environment; the moment this equilibrium is seriously disturbed, the organism ceases to exist as a particular system. Reflexes are the elements of this constant adaptation or equilibration. Physiologists have studied and are studying numerous reflexes, these indispensable, machine-like reactions of the organism, which at the same time are inborn, i.e., determined by the peculiar organization of the given nervous system. Reflexes, like the belts of machines made by human hands, are of two kinds: the positive and the negative inhibitory, in other words, those which excite certain activities and those which inhibit them. Although investigation of these reflexes by physiologists has been under way for a long time, it is, of course, a long way from being finished. More and more new reflexes are being discovered; the properties of the receptor organs,

in which the external and especially the internal stimuli produce certain impulses, still remain in many cases unexplored. The paths along which nervous excitation is conducted within the central nervous system are often little known or not known at all. The central mechanism of inhibitory reflexes, excluding those which manifest themselves along the inhibitory efferent nerves, is quite obscure; the combination and interaction of the various reflexes have not yet been sufficiently elucidated. Nevertheless, physiologists are penetrating deeper and deeper into the mechanism of this machine-like functioning of the organism, and have every reason for believing that sooner or later they will elucidate it in full measure and exercise complete control over it.

Akin to the usual reflexes that have long been the object of physiological investigation in the laboratory and which concern mainly the functions of separate organs, are other inborn reactions; these reactions also take place in the nervous system, and are governed by definite laws, i.e., they are strictly determined by definite conditions. They are the reactions of different animals in relation to the functioning of the organism as a whole, manifested in the general behaviour of the animals and designated by the special term "instincts." Since full agreement has not yet been attained with regard to the essential similarity of these reactions to reflexes, I shall dwell on this question somewhat longer.

Physiology owes to Herbert Spenser, the English philosopher, the first suggestion that instinctive reactions are reflexes too. Afterwards zoologists, physiologists and comparative psychologists produced numerous facts in support of this suggestion. I shall try to systematize the various arguments to the effect that there is not a single essential feature distinguishing reflexes from instincts. First of all there are numerous, imperceptible stages of transition from the usual reflexes to instincts. Take, for example, a newly hatched chick; it reacts by pecking movements to any stimulus in the field of its vision, be it a tiny object or a stain

on the surface on which it is walking. In what way does it differ, say, from inclining the head and closing the lids when something flashes near the eye? We should call the latter a defensive reflex, and the first an alimentary instinct, although in the case of the pecking, if it is caused by the sight of a stain, nothing but inclining the head and a movement of the beak occurs.

Further, it has been noted that instincts are more complex than reflexes. But there are exceedingly complex reflexes which no one designates as instincts. Take, for example, vomiting. This is a highly complex action and one that involves extraordinary co-ordination of a large number of muscles, both striated and smooth, usually employed in other functions of the organism and spread over a large area. It also involves the secretion of various glands which normally participate in quite different activities of the organism.

The fact that instincts involve a long chain of successive actions, while reflexes are, so to speak, one-storeyed, has also been regarded as a point of distinction between them. By way of example let us take the building of a nest, or of animal dwellings in general. Here, of course, we have a long chain of actions: the animal must search for the material, bring it to the site and put it together and secure it. If we regard this as a reflex, we must assume that the ending of one reflex excites a new one, or, in other words, that these are chain-reflexes. But such chain activities are by no means peculiar to instincts alone. We are familiar with many reflexes which are also interlocked. Here is an instance. When we stimulate an afferent nerve, for example, the n. ischiadicus, there takes place a reflex rise of blood pressure. This is the first reflex. The high pressure in the left ventricle of the heart and in the first part of the aorta acts as a stimulus to another reflex: it stimulates the endings of the n. depressoris cordis[45] which evokes a depressor reflex moderating the effect of the first reflex. Let us take the chain-reflex recently established by Magnus. A cat, even

deprived of the cerebral hemispheres will in most cases fall on its feet when thrown from a height. How does this occur? The change in the spatial position of the otolithic organ of the ear causes a certain reflex contraction of the muscles in the neck, which restores the animal's head to a normal position in relation to the horizon. This is the first reflex. The end of this reflex—the contraction of the muscles in the neck and the righting of the head in general—stimulates a fresh reflex on certain muscles of the trunk and limbs which come into action and, in the end, restore the animal's proper standing posture.

Yet another difference between reflexes and instincts has been assumed, namely, that instincts often depend on the internal state or condition of the organism. For instance, a bird builds its nest only in the mating season. Or, to take a simpler example, when the animal is sated, it is no longer attracted by food and stops eating. The same applies to the sexual instinct, which is connected with the age of the organism, as well as with the state of the reproductive glands. In general the hormones, products of the glands of internal secretion, are of considerable importance in this respect. But this, too, is not a peculiar property of the instincts alone. The intensity of any reflex, as well as its presence or absence, directly depends on the state of excitability of the reflex centres which in turn always depends on the chemical and physical properties of the blood (automatic stimulation of the centres) and on the interaction of different reflexes.

Finally, importance is sometimes attached to the fact that reflexes are related to the activity of separate organs, whereas instincts involve the activity of the organism as a whole, i.e., actually the whole skeleto-muscular system. However, we know from the works of Magnus and de Kleyn that standing, walking, and bodily balance in general, are reflexes.

Thus, reflexes and instincts alike are natural reactions of the organism to certain stimulating agents, and conse-

quently there is no need to designate them by different terms. The term "reflex" is preferable, since a strictly scientific sense has been imparted to it from the very outset.

The aggregate of these reflexes constitutes the foundation of the nervous activity both in men and animals. Consequently, thorough study of all these fundamental nervous reactions of the organism is, of course, a matter of great importance. Unfortunately, as already mentioned, this is a long way from having been accomplished, especially in the case of those reflexes which are called instincts. Our knowledge of these instincts is very limited and fragmentary. We have but a rough classification of them—alimentary, self-defensive, sexual, parental and social. But almost each of these groups often includes numerous separate reflexes, some of which have not been even identified by us, while some are confused with others or, at least, they are not fully appreciated by us as to their vital importance. To what extent this subject remains unelucidated and how full it is still of gaps can be demonstrated by this example from my own experience.

Once, in the course of our experimental work which I shall describe presently, we were puzzled by the peculiar behaviour of our animal. This was a tractable dog with which we were on very friendly terms. The dog was given a rather easy assignment. It was placed in the stand and had its movements restricted only by soft loops fastened round its legs (to which at first it did not react at all). Nothing else was done except to feed it repeatedly at intervals of several minutes. At first the dog was quiet and ate willingly, but as time went on it became more and more excited: it began to struggle against the surrounding objects, tried to break loose, pawing at the floor, gnawing the supports of the stand, etc. This ceaseless muscular exertion brought on dyspnoea and a continuous secretion of saliva; this persisted for weeks, becoming worse and worse,

with the result that the dog was no longer fit for our experimental work. This phenomenon puzzled us for a long time. We advanced many hypotheses as to the possible reason for this unusual behaviour, and although we had by then acquired sufficient knowledge of the behaviour of dogs, our efforts were in vain until it occurred to us that it might be interpreted quite simply—as the manifestation of a freedom reflex, and that the dog would not remain quiet so long as its movements were constrained. We overcame this reflex by means of another—a food reflex. We began to feed the dog only in the stand. At first it ate sparingly and steadily lost weight, but gradually it began to eat more—until it consumed the whole of its daily ration. At the same time it became quiet during the experiments; the freedom reflex was thus inhibited. It is obvious that the freedom reflex is one of the most important reflexes, or, to use a more general term, reactions of any living being. But this reflex is seldom referred to, as if it were not finally recognized. James does not enumerate it even among the special human reflexes (instincts). Without a reflex protest against restriction of an animal's movements any insignificant obstacle in its way would interfere with the performance of certain of its important functions. As we know, in some animals the freedom reflex is so strong that when placed in captivity they reject food, pine away and die.

Let us turn to another example. There is a reflex which is still insufficiently appreciated and which can be termed the investigatory reflex. I sometimes call it the "What-is-it?" reflex. It also belongs to the fundamental reflexes and is responsible for the fact that given the slightest change in the surrounding world both man and animals immediately orientate their respective receptor organs towards the agent evoking the change. The biological significance of this reflex is enormous. If the animal were not provided with this reaction, its life, one may say, would always hang by a thread. In man this reflex is highly developed, manifesting itself in the form of an inquisitiveness which gives birth

to scientific thought, ensuring for us a most reliable and unrestricted orientation in the surrounding world. Still less elucidated and differentiated is the category of negative, inhibitory reflexes (instincts) induced by any strong stimuli, or even by weak but unusual stimuli. So-called animal hypnotism belongs, of course, to this category.

Thus, the fundamental nervous reactions both of man and animals are inborn in the form of reflexes. And I repeat once more that it is highly important to have a complete list of these reflexes and properly to classify them, since, as we shall see later, all the remaining nervous activity of the organism is based on these reflexes.

However, although the reflexes just described constitute the fundamental condition for the safety of the organism in the surrounding nature, they in themselves are not sufficient to ensure a lasting, stable and normal existence for the organism. This is proved by the following experiment, carried out on a dog in which the cerebral hemispheres have been extirpated. Besides the internal reflexes, such a dog retains the fundamental external reflexes. It is attracted by food; it keeps away from destructive stimuli; it displays the investigatory reflex pricking up its ears and lifting its head to sound. It possesses the freedom reflex as well, and strongly resists any attempt at capture. Nevertheless, it is an invalid and would not survive without care. Evidently something vital is missing in its nervous activity. But what? It is impossible not to see that the number of stimulating agents evoking reflex reactions in this dog has decreased considerably, that the stimuli act at a very short distance and are of a very elementary and very general character, being undifferentiated. Hence, the equilibrium of this higher organism with the environment in a wide sphere of its life has also become very elementary, limited and obviously inadequate.

Let us now revert to the simple example with which we began our investigations. When food or some unpalatable substance gets into the mouth of the animal, it evokes a

secretion of saliva which moistens, dissolves and chemically alters the food, or in the case of disagreeable substances removes them and cleanses the mouth. This reflex is caused by the physical and chemical properties of the above-mentioned substances when they come in contact with the mucous membrane of the oral cavity. However, a similar secretory reaction is produced by the same substances when placed at a distance from the dog and act on it only by appearance and smell. Moreover, even the sight of the vessel from which the dog is fed suffices to evoke salivation, and what is more, this reaction can be produced by the sight of the person who usually brings the food, even by the sound of his footsteps in the next room. All these numerous, distant, complex and delicately differentiated stimuli lose their effect irretrievably when the dog is deprived of the cerebral hemispheres; only the physical and chemical properties of substances, when they come in contact with the mucous membrane of the mouth, retain their effect. Meanwhile, the processing significance of the lost stimuli is, in normal conditions, very great. Dry food immediately encounters plenty of the required liquid; unpalatable substances, which often destroy the mucous membrane of the mouth, are removed from it by a layer of saliva rapidly diluted and so on. But their significance is still greater when they bring into action the motor component of the alimentary reflex, i.e., when the seeking of food is effected.

Here is another important example of the defensive reflex. The strong animals prey on those smaller and weaker, and the latter must inevitably perish if they begin to defend themselves only when the fangs and claws of the enemy are already in their flesh. But the situation is quite different when the defensive reaction arises at the sight and sound of the approaching foe. The weak animal has a chance of escaping by seeking cover or in flight.

What, then, would be our general summing up of this difference in attitude of the normal and of the decorticated

animal to the external world? What is the general mechanism of this distinction and what is its basic principle?

It is not difficult to see that in normal conditions the reactions of the organism are evoked not only by those agents of the external world that are essential for the organism, i.e., the agents that bring direct benefit or harm to the organism, but by other countless agents which are merely signals of the first agents, as demonstrated above. It is not the sight and sound of the strong animal which destroy the smaller and weaker animal, but its fangs and its claws. However, the signalling, or to use Sherrington's term, the distant stimuli, although comparatively limited in number, play a part in the afore-mentioned reflexes. The essential feature of the higher nervous activity, with which we shall be concerned and which in the higher animal is probably inherent in the cerebral hemispheres alone, is not only the action of countless signalling stimuli, rather it is the important fact that in certain conditions their physiological action changes.

In the above-mentioned salivary reaction now one particular vessel acted as a signal, now another, now one man, now another—strictly depending on the vessel that contained the food or the unpalatable substances before they were introduced in the dog's mouth, and which person brought and gave them to the dog. This, clearly, makes the machine-like activity of the organism still more precise and perfect. The environment of the animal is so infinitely complex and is so continuously in a state of flux, that the intricate and complete system of the organism has the chance of becoming equilibrated with the environment only if it is also in a corresponding state of constant flux.

Hence, the fundamental and most general activity of the cerebral hemispheres is signalling, the number of signals being infinite and the signalization variable.

LECTURE TWO

Technical methods used in objective study of the work of the cerebral hemispheres. Signalling as a reflex action. Unconditioned and conditioned reflexes. Conditions for the development of conditioned reflexes.

Gentlemen,

In the previous lecture I touched on the reasons that impelled us to adopt a strictly objective method of investigating the entire nervous activity in higher animals, i.e., of studying it from the purely external factual aspect; this is in keeping with the investigations in other branches of natural science, and rules out fantastic speculation as to the probable subjective state of the animals by analogy with ourselves. At the same time I stated that from this point of view the entire nervous activity of the animal appeared to us first of all as inborn reflexes, i.e., regular connections between certain external agents acting on the organism and its definite responsive functions. It has been established that these agents are comparatively few in number; they prove to be akin to each other, being of a general nature. Of course, this, to a degree, ensures the existence of the organism, but it is far from being sufficient (especially in the more highly developed animals); therefore, if we deprive an animal of a certain part of its nervous activity, it will be fully disabled and, if left to itself, although retaining its inborn reflexes, doomed to death. Everyday life demands more detailed and specialized correlations between the animal and the surrounding world. And this further correlation is established only with the help of the higher part of the central nervous system—the cerebral hemispheres; to be more precise, a large number of natural stimuli act as temporary and alternate signals for the relatively small number of fundamental agents that determine the inborn reflexes. Only in this way is a precise and delicate equilibration of the organism with the environment attained. I have designated this function of the cerebral hemispheres signalling activity.

Now I should like to touch on the technical side of the methods used by us in our investigation. How shall we study the signalling activity of the hemispheres—which organ shall we select and what methods shall we use? It is obvious that any reflex can be chosen for investigation since signalling stimuli are connected with all the reflexes. But, as already mentioned, the particular conditions of our work made us concentrate on the study of two reflexes—the alimentary reflex and the usual defensive reflex which manifested itself when an unpalatable substance was introduced into the mouth of the dog used in our experiment. This was a fortunate choice in many respects. Whereas a strong defensive reflex, evoked, for example, by the application of an electric current to the skin, greatly excites the animal and makes it restless, and while the sexual reflex requires special conditions (to say nothing of its protracted periodic character and dependence on age), the alimentary reflex and the mild defensive reflex to unpalatable substances introduced into the mouth are normal and ordinary phenomena.

Here is another essential feature of our method of investigation. The alimentary reflex, and the reaction to unpalatable substances when they find their way into the animal's mouth, each consist of two components. On the one hand, the animal takes the food, masticates it and swallows it, or, in the case of unpalatable substance, ejects it from the mouth. On the other hand, this muscular activity is joined by a secretory one. Both the food and the unpalatable substances evoke an immediate secretion of saliva, needed in the first case for the physical and chemical processes of digestion, and in the second case for cleansing the mouth. In our experiments we used only the secretory component of the reflexes, the motor reactions being taken into account only in special circumstances. The secretory reflex proved most suitable for our experiments, since it is possible to take accurate measurements of the intensity of salivation, either by counting the number of drops or by means of a

special graduated tube. It would be much more difficult to obtain accurate measurements for the motor component of the reflexes, which in this case is of a very complex and diversified character. The most delicate instruments would be required for this purpose, and even then they would not give the precision in measuring the motor reaction that is achieved in the case of the secretory component. Of certain importance in the early phase of our work was the fact that during observations on the secretion of saliva there was a lesser tendency towards anthropomorphic interpretations than was the case during observations of the motor reactions.

All the dogs used in our experiments are preliminarily subjected to a minor operation which consists in transplanting the opening of the salivary duct to the external surface of the skin. For this purpose we cut out a small piece of the mucous membrane surrounding the natural opening of the salivary duct in the mouth, then we separate the duct for a certain depth, bring out its end to the external surface of the skin through a special incision in the cheek and suture it to the edges of the incision. As a result of this operation the saliva flows not into the mouth, but outside, on to the cheek or under the chin. This greatly facilitates observation of the work of the salivary glands. It is only necessary to fix a small glass funnel with the help of cement (we utilize Mendeleyev's cement for the purpose), and the work of the glands can be observed in different ways and with remarkable exactness. Sometimes a hemispherical bulb is hermetically adjusted by us over the fistula; the bulb is provided with two projecting tubes, one pointing up and the other down. The lower tube is used for drawing off the saliva which accumulates after each stimulation, while the upper tube is connected by air transmission with a horizontal glass tube filled with coloured fluid. When the saliva flows into the hemispherical bulb it displaces the coloured fluid which begins to move along the graduated tube, and thus the amount of secretion is

accurately recorded. It is also easy to fix up an automatic electric device accurately recording the number of drops of exactly equal volume.

Now I shall turn to the general conditions of our experiments. Since our research deals with the activity of the cerebral hemispheres, a highly complex signalling apparatus of the finest sensitiveness, it is obvious that numerous and diverse stimuli constantly act on the animal through it. Each of these stimuli has a certain effect on the animal and in the aggregate they collide and interact. Consequently, if we do not take precautions against these influences, often of a chaotic character, we should not be able properly to understand the phenomena under investigation, everything being confused and entangled. It is, therefore, important to simplify the conditions of the experiment. First of all the animal is usually placed in a stand. At first the experimenter alone was allowed to remain with the dog in the research chamber. But this precaution proved insufficient, since the experimenter himself is a source of numerous stimuli. His slightest movement, respiration, eye movements, etc., all act on the animal undergoing the experiment and interfere with the phenomena under investigation. Therefore, we were compelled to station the experimenter beyond the research chamber so as to exclude as much as possible his influence on the animal. But even this precaution proved inadequate in the conditions of an ordinary laboratory. Actually, the environment of the dog in an ordinary laboratory is constantly changing: sounds penetrate from the outside, footsteps, the noise of street traffic, a chance conversation, vibration of the walls caused by a passing van, shadows cast through the windows, and so on—any of these extraneous casual stimuli falling on the cerebral hemispheres must be reckoned with. For this reason a special laboratory was built at the Institute of Experimental Medicine, the necessary funds being provided by an enlightened Moscow businessman. The primary task was to prevent as much as possible the access of extrane-

ous stimuli. For this purpose the laboratory was surrounded with a deep trench, and other structural devices were used. Inside, the rooms (four on each floor) were isolated by a cross-shaped corridor; the top and ground floors, housing the working rooms, were separated by an intermediate floor. Finally, each research chamber was thoroughly partitioned by means of certain sound-proof materials into two compartments—one for the animal and the other for the experimenter. Pneumatic or electric transmission was used for the purpose of stimulating the animal and recording its reactions. Thus, the maximum simplification and stability of the experimental conditions were ensured.

Lastly, one other point should be mentioned, which to a considerable degree still remains a pium desiderium. Since the entire complex of external influences on the animal is subjected to investigation, it is understandable that this complex must be fully under the control of the experimenter. He must have at his disposal a large number of instruments of various kinds in order to act on the animal by different kinds of stimuli and to combine certain stimuli so as to reproduce actual natural conditions. But we often experienced and still experience a shortage not only of specially perfected instruments but of modern instruments used in research. The functioning of the cerebral hemispheres each time proves too delicate to be fully investigated with the instruments at our disposal.

It may be that someone will say that the experimental conditions just described by me are artificial. Our reply to such an objection would be this. Firstly, in view of the infinite variety of living relations it is hardly possible to use something really unusual and absolutely new. Secondly, in investigating phenomena of a chaotic and complex nature, we must inevitably break them up and divide them into groups. Has not the physiology of animals constantly employed and is it not employing now the methods of vivisection and even the method of isolated organs and tissues? We subject our animal to a restricted number of

definite conditions, and thus make it possible to study their influences independently of one another. You will see later how the conditions of our experimentation and the variations in the state of the animal furnished us with facts of vital importance.

Such are our general principles and the technical side of our methods.

Let us pass now to the subject of the signalling activity of the cerebral hemispheres itself. Here are a few demonstrations:

Demonstration. The animal used in this experiment has been operated upon as previously described. As you see, so long as no special stimulating agent acts on the animal, its salivary glands remain inactive and there is no secretion of saliva. Now we begin to act upon the dog's ear by beating a metronome. Salivary secretion begins in nine seconds, and in the course of forty-five seconds eleven drops have been secreted. Thus, before your eyes a stimulus utterly alien to food (the metronome) has evoked the activity of the salivary gland, and this activity must be regarded as a component of the alimentary reflex. You have also seen the motor component of this reflex: the dog turned in the direction from which the food is usually brought and began to lick its lips.

It is this central phenomenon, originating in the cerebral hemispheres, that will be the object of our further attention. A decorticated dog would never have reacted by a secretion of saliva to a stimulus of this kind. At the same time it is quite obvious that this is a signalling activity: the beats of the metronome are signals for food, since the animal reacts to them in the same way as if it were food. A similar effect is produced by the sight of real food.

Demonstration. Food is shown to the animal. As you see, the secretion of saliva begins after five seconds, and in fifteen seconds six drops of saliva have been collected. Here, then, we have the same effect as observed under the action of the metronome.

This is also a case of signalling due to the activity of the cerebral hemispheres; it has been acquired in the course of the animal's individual existence and is by no means an inborn reaction. This was revealed by the experiments conducted by I. S. Tsitovich in the laboratory of the late Prof. V. I. Vartanov.[46] Tsitovich took a number of pups from their mother and for quite a while fed them exclusively on milk. When they were a few months old, he performed fistula operations on their salivary ducts and in this way he was able to record the secretion of saliva. When the pups were shown other food, namely, meat and bread, no salivary secretion was observed. Consequently, the sight of food does not, in itself, evoke a salivary reaction, and does not represent an inborn agent of this reaction. Only after the pups had had several meals of meat and bread did the sight of these items produce a salivary secretion.

The following experiment will demonstrate a phenomenon generally known as a reflex.

Demonstration. We suddenly introduce food into the dog's mouth and the secretion of saliva begins after a second or two. This is due to the action of the mechanical and chemical properties of the food on the mucous membrane of the mouth; it is a reflex. This experiment explains why a decorticated dog may die of starvation in the presence of food; it will begin to eat only when the food comes into contact with its mouth.

This experiment brings out the insufficiency of the inborn reflexes, their imperfection and their limited character; at the same time it brings out clearly the great importance of signals.

Now comes the fundamental question: what is the nature of signalization and how should it be considered from the purely physiological point of view?

We know that a reflex is an indispensable, natural reaction of the organism to an external agent effected by a definite part of the nervous system. It is quite obvious that in signalization there are present all the components of the

nervous activity which is called a reflex. An external stimulus is required for the development of a reflex; in our first experiment this stimulus was provided by the beats of a metronome. They set in motion the auditory receptor of the dog, and the stimulation was further transmitted along the auditory nerve to the central nervous system; here it was transferred to the nerves of the salivary glands which excited a secretion of saliva. It is true that in the experiment with the metronome we observed an interval of nine seconds between the beginning of the action of the metronome and the beginning of the salivary secretion, whereas for the reflex this interval was only one to two seconds. But the longer latent period was due to special conditions deliberately created by us in the course of the experiment. Generally, the effect produced by signalization far from being belated, is evoked as quickly as that induced by ordinary reflexes; this question, however, will be discussed in another lecture. A reflex is a reaction strictly determined by definite conditions. The same holds true for signalization, the only difference being that in the latter case the effect depends on the greater number of conditions. But this, of course, does not make signalization differ fundamentally from reflexes, since in strictly definite conditions reflexes likewise often vanish, are inhibited. A thorough study of the subject shows that there is nothing accidental in the signalling activity of the hemispheres; here, too, the experiments are carried out strictly in accordance with our designs. In the special laboratory I have described, it often happens that the animal is under observation for one or two hours without secreting a single drop of saliva not caused by the stimuli applied; in the ordinary laboratories, of course, the experiments are often distorted by extraneous stimuli.

All these facts leave no grounds for regarding the phenomena, which up to now I have designated "signalization," as being other than reflexes. But there is another aspect which, at first sight, would seem to indicate an

essential difference between the old reflex and this new phenomenon, which only a moment ago I also termed a "reflex." Food, through its mechanical and chemical properties, evokes the salivary reflex in any animal right from birth. But this new type of reflex, which you have seen illustrated, is developed gradually in the course of the animal's individual existence. Can this be regarded as an essential difference? Is it not an argument against terming this new phenomenon a reflex? Undoubtedly, it is a sufficient argument for distinguishing this type of reaction from the other, but by no means does it annul our scientific right to term it a reflex. Here it is a question not of the mechanism itself, but of the mode of formation of the reflex mechanism. Let us, by way of example, take the telephone system. Communication can be effected in two ways. My apartment can be connected directly with the laboratory by a special line which enables me to put a call through whenever I like, or, as is actually the case, a connection with the laboratory may be established through the central telephone exchange. But the result is the same, the only difference being that in the first case there is a readily available conducting line, while in the second a preliminary connection is required; in one case the mechanism effecting the communication is ready-made, in the other—it must be supplemented every time to make it complete. The same holds true for the reflex action: in the one case it is complete, in the other—a certain preliminary preparation is required.

Thus, we have to consider the question of the mode of formation of the new reflex mechanism. Since the new reflex develops easily and unfailingly in definite physiological conditions—a fact which will be illustrated later—there are no grounds for worrying about the subjective state of the dog. With complete knowledge of the matter, this phenomenon is fully under our control; it is strictly law-governed and there is no reason to regard it as being

other than physiological activity, similar to all other activities with which physiology is concerned.

We have termed the new reflexes *conditioned* in contrast to the *unconditioned* inborn reflexes. The term "conditioned" is, more and more, coming into general use. From the point of view of research, it is fully justified, since in comparison with the inborn, these reflexes are conditioned in a special way: in the first place their formation requires definite conditions, and, in the second place, their action also depends on numerous conditions. Consequently, when investigating them, the researcher must take very many factors into account. Of course, our terms could, with justification, be replaced by others. Thus, for example, the old reflexes might be called "inborn" and the new ones—"acquired"; we could also term the former "species reflexes" since they are typical of the species as a whole, and the latter "individual reflexes," since they vary in different animals, and even in one and the same animal at different times and in different conditions. The terms "conductor reflexes" and "connection reflexes" would, likewise, be fully justified.

As for the assumption of the formation of new nervous connections within the cerebral hemispheres, there should be no objection to it from the theoretical point of view. The principle of connection is so often applied in modern technique, as well as in our everyday experience, that it would seem strange if it were regarded as an unexpected one in the mechanism of the central nervous system, the function of which is to establish highly complex and delicate relations. It is perfectly natural that along with the conductor mechanism there should also be a connector mechanism. The physiologist of all people should have no objection to this concept, especially since decades ago there was in general use the German concept "Bahnung"[17] which means laying down new paths, establishing new connections. The conditioned reflex is a common and widespread phenomenon. It is, evidently, what we recognize in ourselves and

in animals under such names as training, discipline, education, habits; these are nothing but connections established in the course of individual existence, connections between definite external stimuli and corresponding reactions. Thus, the conditioned reflex opens to the physiologist the door to investigation of a considerable part, and possibly, even of the entire higher nervous activity.

I shall pass now to the question of the circumstances in which conditioned reflexes, or connections of new nervous paths are established. The fundamental requisite is that the external stimulus must coincide in time with the action of the unconditioned stimulus. In our experiment food acted as the unconditioned stimulus of the alimentary reaction. If the intake of food by the animal coincides in time with the action of a stimulus which previously had no relation to food, this stimulus begins to produce the same reaction as the food. Precisely this occurred in the case which passed before our eyes. We repeatedly stimulated the dog undergoing the experiment by the sound of the metronome and then fed the animal immediately, i.e., evoked the inborn alimentary reflex. After several repetitions the very sounds of the metronome began to produce a secretion of saliva and a corresponding motor reaction. The same occurs in the case of a defensive reflex to unpalatable substances introduced into the dog's mouth. If we introduce a weak acid solution we get an unconditioned acid reflex: the animal begins to make various movements, shaking its head violently, opening its mouth, trying to eject the acid with the help of the tongue, and so on; at the same time it manifests a profuse secretion of saliva. Exactly the same reaction is caused by any external stimulus repeatedly coinciding in time with the introduction of acid into the dog's mouth. *Thus, the first and foremost requisite for the formation of a conditioned reflex lies in the coincidence in time of the action of a previously indifferent agent with the action of an unconditioned agent which evokes a definite unconditioned reflex.*

The second important requisite is that the *conditioned reflex can be formed only if the indifferent agent somewhat precedes the action of the unconditioned stimulus.* If the order is reversed, i.e., if we apply the unconditioned stimulus first, and the indifferent agent afterwards, there will be no conditioned reflex.

A. N. Krestovnikov carried out a variety of experiments of this kind in our laboratory, and the effect was invariably the same. Here are some of his results. In the case of one dog 427 combinations of the odour of vanilin with the introduction of acid into the mouth were applied, the acid always preceding the odour of vanilin by five to ten seconds. Vanilin did not become a conditioned stimulus to an acid reaction. However, in subsequent experiments the odour of amyl acetate, which *preceded* the introduction of acid into the dog's mouth, became an effective conditioned stimulus after only twenty combinations. With another dog the sound of a loud electric bell, set buzzing five to ten seconds after it began to take food, did not produce a conditioned alimentary reaction after 374 combinations; but the rotation of an object in front of the dog prior to the administration of food, acquired the properties of a conditioned stimulus after only five combinations. Later, when the electric bell was rung *prior* to the administration of food, it also became a conditioned stimulus after a single combination. These experiments were tried on five dogs, and the result was always the same, no matter whether the new stimulating agent was applied ten seconds, five seconds or only two and even one second after the onset of the unconditioned stimulus. During the elaboration of conditioned reflexes, for the sake of greater certitude, we carefully observed not only the secretory but also the motor reactions of the animals. Thus, the first set of conditions includes the time relation between the unconditioned stimulus and the agent which becomes the conditioned stimulus.

As for the cerebral hemispheres themselves, only their active state makes possible the formation of conditioned

reflexes. If the animal undergoing the experiment is in a more or less drowsy state, the formation of the conditioned reflex either acquires a protracted character, being considerably impeded, or becomes completely impossible. Consequently, the establishment of new connections, the process of coupling new nervous paths is a function which can be performed only when the animal is in an alert state. At the same time during the formation of a new conditioned reflex the cerebral hemispheres must be free from all other activity.

When elaborating a new conditioned reflex it is important to prevent extraneous stimuli from affecting the animal, since they are liable to give rise to altogether different reactions of the organism. If corresponding precautions are not taken, the formation of a conditioned reflex becomes exceedingly difficult and in many cases utterly impossible. For example, if during the elaboration of a conditioned reflex the dog is strongly irritated by a certain part of the stand in which it is fastened (causing pressure, strangulation, etc.), then no matter how many times we repeat the combination of our stimulus with the unconditioned stimulus, or at any rate with some of them, we shall not get a conditioned reflex. Another example is provided by the dog already mentioned which would not keep quiet when constrained in the stand. Hence the rule, almost without exception: in a new experimental animal, i.e., one which has not yet been subjected to the experiments in question, the establishment of the first conditioned reflex is difficult and often takes much time. This is obvious, since the experimental conditions themselves may provoke in different animals numerous special reactions, i.e., cause one or other extraneous activity of the cerebral hemispheres. It should be added that in cases when we are not in a position to disclose the nature of the extraneous reflexes that interfere with the formation of our conditioned reflex and when we are unable to get rid of them, the inherent properties of the nervous activity come to our aid. For if the environment of the animal dur-

ing the experiment does not contain any particularly destructive elements, then almost all extraneous and interfering reflexes will gradually vanish of themselves.

Of course, this set of conditions also includes the state of health of the animal; sound health ensures the normal functioning of the cerebral hemispheres and precludes any influence of internal pathological stimuli coming into the cerebral hemispheres.

Finally, the last set of conditions relates to the properties of the agent which is to become a conditioned stimulus, as well as to the properties of the given unconditioned stimulus.

Conditioned reflexes are easily elaborated from more or less indifferent agents. Strictly speaking, there is no such thing as an absolutely indifferent agent. In a normal animal the slightest change in the environment—even the faintest sound, odour, or change in the lighting of a room—immediately evokes the above-mentioned investigatory, "What-is-it?" reflex in the shape of a corresponding motor reaction. But if this relatively indifferent agent recurs, then it rapidly loses its effect on the cerebral hemispheres, and the obstacle to the formation of a conditioned reflex is thereby removed. But if the agent belongs to the group of strong general stimuli, or, moreover, of special stimuli, the formation of our conditioned reflex will be greatly impeded and in extreme cases even impossible. It should also be borne in mind that in most cases the previous history of the dog was not known to us, we knew little of its life, the conditioned connections established in it, etc. On the other hand, we used as an agent even a strong unconditioned stimulus and were successful in transforming it into a conditioned stimulus. Take, for example, such a destructive stimulus as a strong electric current applied to the skin and causing injury and cauterization. This, obviously, is an unconditioned stimulus to the defensive reflex: the organism responds by a violent motor reaction directed either towards removal of the stimulus itself or to moving away from it.

Nevertheless, even by means of such stimuli it is possible to elaborate other kinds of conditioned reflexes.

In one experiment this destructive stimulus was converted into an alimentary conditioned stimulus. When an extra strong electric current was applied to the skin of the dog, the latter did not display even the slightest defensive reaction; actually there was in evidence an alimentary reaction: the animal turned in the direction whence it usually received its food, licking its lips and exhibiting an abundant secretion of saliva.

Here is the original record of an experiment carried out by M. N. Yerofeeva.

Time	Electric current (distance between coils in cm.)	Place of skin stimulated	Secretion of saliva in drops during 30 seconds	Motor reaction
4.23 p. m.	4	Usual place	6	Alimentary
4.45 „	4	„ „	5	No trace of
5.07 „	2	New place	7	defensive
5.17 „	0	„ „	9	reaction
5.45 „	0	„ „	6	

After each electric stimulation the dog was allowed to eat food for a few seconds.

A similar effect was obtained in a dog whose skin was repeatedly subjected to cauterization or to pricking deep enough to draw blood. When sensitive people expressed indignation at these experiments we were in a position to show that they were mistaken. Of course here, too, we had no intention of penetrating into the subjective world of the dog and of ascertaining its feelings. But we had absolutely reliable proof that even the most delicate objective phenomena usually manifested in the animals when the latter are subjected to the action of strong and destruc-

tive stimuli, were not observed in this particular case. In our dogs, in which the reflexes had been transformed as described above, no appreciable change in the pulse or in the respiration was evoked by the stimulation, whereas such changes inevitably occur and assume a pronounced character when the destructive stimulus is not preliminarily associated with an alimentary reaction. Such is the remarkable result of diverting the nervous excitation from one path to another. But this transformation of reflexes depends on a definite condition—namely, a certain correlation between the two unconditioned reflexes. The conversion of the unconditioned stimulus for one reflex into the conditioned stimulus for another is possible only when the first reflex is physiologically weaker and biologically of less importance than the second. This conclusion, I believe, can be drawn from the further results obtained by Yerofeeva. A destructive stimulus applied to the dog's skin was transformed into a conditioned alimentary stimulus. But this, we think, was due to the fact that in the case of the damage to the skin, the alimentary reflex proved stronger than the defensive reflex. We know that dogs, when fighting for food, often sustain skin wounds, which means that the alimentary reflex predominates over the defensive reflex. But to this, too, there is a limit. There is a reflex that is stronger than the alimentary reflex—the life and death reflex, to be or not to be. This explains the following phenomenon observed in the course of our experiments: a strong electric current applied to skin overlying bone, without any intervening thick muscular layer, could not be converted into a conditioned stimulus for an alimentary reaction instead of a defensive reaction. This signifies that the afferent nerves, stimulated by the damage to the bone and signalizing a grave danger to the organism, establish with great difficulty, or fail to do so at all, a temporary connection with the part of the brain from which the alimentary reaction is controlled. It should be said in passing that the foregoing facts clearly show the advantage of employing the

unconditioned alimentary reflex for our experiments, since it occupies a very high place in the hierarchy of reflexes.

On the one hand, as we have just seen, strong and even specialized agents become, in certain conditions, conditioned stimuli; on the other hand, there is, of course, a minimum strength below which the agent cannot function as a conditioned stimulus. Thus, for example, a thermal agent below 38°-39° C. applied to the skin cannot be made into a conditioned thermal stimulus (experiments of O. S. Solomonov).

Similarly, if by using such a very strong unconditioned stimulus as food—as was the case in our experiment—it is possible to transform a most unfavourable agent, already part of another reflex, even an unconditioned one, into a conditioned stimulus, the use of a weak unconditioned stimulus makes it extremely difficult or even impossible to transform even a favourable, i.e., almost indifferent agent, into a conditioned stimulus; even when such a conditioned stimulus is formed, it is inconsiderable. These are either constantly weak or temporarily weak unconditioned stimuli which, given other states of the organism, could be, on the contrary, very strong. Food can be taken as an example: in a hungry animal food naturally evokes a strong unconditioned alimentary reflex, and in this case the conditioned reflex develops rapidly and is also of considerable strength. In a permanently satisfied animal the unconditioned reflex is less powerful, and the conditioned reflex either is not formed at all or is established very slowly.

By complying with the conditions enumerated above—which is not a difficult task—we obtain a conditioned reflex *without fail*. Why, then, should we not regard the formation of a conditioned reflex as a purely physiological phenomenon? We act on the dog's nervous system by means of a number of definite external stimuli, and they, *necessarily*, result in the establishment of a new nervous

connection; a certain nervous coupling takes place and, as shown above, a typical reflex reaction follows. Where, then, is there any place for any kind of non-physiological relations? Why, then, are the conditioned reflex and the laws governing its formation not regarded as physiology, but as something else? I see no reason for thinking about these phenomena in any other way, and it is my belief that human prejudice usually plays a harmful role in these questions by its reluctance to admit that the higher nervous activity is strictly determined; this is because of the extraordinary complexity of our subjective experiences, of our actions which in most cases at present cannot be traced to their ultimate definite stimuli.

NATURAL SCIENCE AND THE BRAIN[48]

One can truthfully say that for the first time since the days of Galileo the irresistible march of natural science has been held up quite perceptibly before the study of the higher parts of the brain, the organ of the highly complex relationship between the animal and the external world. And it would seem that this is not fortuitous, that this is indeed a critical moment in natural science, since the brain, which in its higher form—the human brain—created and is continuing to create natural science, itself becomes the object of this science.

But let us approach the matter more closely. For long the physiologist has persistently and systematically, in keeping with the strict rules of natural science, studied the animal organism. He has observed the vital phenomena unfolding before him in time and space; he endeavours with the help of experimentation to define the constant and elementary conditions of their existence, their coming and going. His foresight and his control over vital phenomena are increasing all the time, in the same way as natural science rises in all its grandeur over inanimate nature before our very eyes. When the physiologist deals with the basic functions of the nervous system—with the processes of nervous excitation and conduction—even though the nature of these phenomena is still obscure, he remains a naturalist, investigating one by one the varied external influences on these general nervous processes. Moreover, when the physiologist studies the lower part of the central nervous system, with the spinal cord, and finds out how the organ-

ism by means of this part responds to these or to other external influences, i.e., when he studies the law-governed changes taking place in the living substance under the action of one or another external agent, he remains exactly the same naturalist. This natural reaction of the animal organism to the external world, effected through the lower part of the central nervous system, is termed by the physiologist a reflex. The reflex, as one would expect, is strictly specific from the point of view of natural science; a certain external phenomenon causes strictly definite changes in the organism.

But when the physiologist turns to the higher levels of the central nervous system, a sudden and abrupt change takes place in his research. He no longer concentrates on the connection between the external phenomena and the animal's reactions to them; instead of dealing with these actual relations he begins to make suppositions about the internal states of the animals modelled on his own subjective state. So far he has based himself on the general concepts of natural science. But he now resorts to concepts that are utterly alien to him and in no way related to his earlier, psychological concepts; in short, he makes the leap from the measurable world to the immeasurable. This, obviously, is a step of extraordinary importance. But what caused it? What profound reasons impelled our physiologist to do this? What conflict of opinions preceded it? A totally unexpected answer must be given to these questions: in the world of science absolutely nothing preceded this extraordinary step. The natural scientist, in the person of the physiologist, investigating the higher parts of the central nervous system, has, so to speak, unconsciously and imperceptibly for himself, yielded to the common habit of regarding the animal's activity as analogous to his own and of explaining it by the same intrinsic causes which he feels and recognizes in himself.

This, then, is the point at which the physiologist departed from the firm position of natural science. And what

has he acquired instead? He has borrowed concepts from that branch of human intellectual interest which, as those who work in this field readily admit, is not yet entitled to call itself science, despite its long existence. Psychology, as the knowledge of the human inner world, is still seeking its own true methods. And the physiologist has taken upon himself the ungratifying task of guessing the inner world of the animal.

After this one can easily understand why the study of the most complex nervous activity of the higher animals is hardly making any progress, although research has been carried on for about a hundred years. In the early seventies of the last century work on the higher part of the brain received what seemed to be a powerful impetus towards further development, but even this failed to bring research on to the highroad of science. A few basic facts were discovered in the first few years and then progress came to a standstill again. The subject, clearly, covers a vast field, and yet one and the same themes have been worked and reworked for the past thirty years and more, and hardly any new ideas have appeared. The impartial present-day physiologist is forced to admit that the physiology of the brain is now in a blind alley. Thus, psychology, as an ally, has not justified itself in the eyes of physiology.

In view of this state of affairs common sense demands that here, too, physiology should return to the path of natural science. But what must it do then? In investigating the activity of the higher parts of the central nervous system it must remain faithful to the methods that it uses in studying the lower parts, i.e., it must strictly compare the changes in the external world with the corresponding changes in the animal organism and disclose the laws governing these relations. But these relations are, apparently, intricate in the extreme. Is it possible to begin to record them objectively? To this really fundamental question there is but one serious answer: persevering and in-

cessant effort is needed in this direction. An exclusively objective comparison of the external world and of the animal organism is now being attempted by a number of investigators on a great variety of species of animals.

I have the honour to submit for your esteemed attention an attempt to investigate the most complex activity of one of the higher animals, namely, the dog. Later on in my exposition I shall base myself on the results of ten years' research in my laboratories where I have been joined by a number of young scientists who are trying their luck in this new field of investigation. This decade of research, at first overshadowed by painful doubts, and then with growing frequency encouraged by the firm feeling that our efforts were not in vain, offers, as I am now convinced, an indisputable and positive answer to the above question.

All the activity of the higher parts of the nervous system, which was revealed before our eyes, appeared to us in the form of two main nervous mechanisms: first, the mechanism of a temporary connection, as it were, a temporary coupling of the conductor paths between the phenomena of the external world and the responsive reactions of the animal organism; secondly, the mechanism of analysers.

Let us consider these mechanisms separately.

I have already mentioned that long ago physiology established in the lower part of the central nervous system the mechanism of the so-called reflex, i.e., of a constant connection effected by the nervous system between certain phenomena of the external world and the corresponding definite reactions of the organism. Since this connection is simple and of a constant nature, it was natural to term it an unconditioned reflex. On the basis of our facts we came to the conclusion that temporary connection is effected in the higher part of the nervous system. By means of this part of the nervous system the phenomena of the external world are now reflected in the activity of the organism, i.e., excite the organism to activity, and now indifferent to the organism and inconvertible, just as if they did not exist at all.

This temporary connection, these new reflexes were, naturally, termed conditioned reflexes. In what way does the organism benefit by the mechanism of temporary connection? When does the temporary connection, the conditioned reflex, appear? Let us take an actual example. The most essential connection between the animal organism and the surrounding world is that brought about by certain chemical substances which constantly enter into the composition of the given organism, i.e., the food connection. In the lower forms of the animal world it is the direct contact between food and the animal organism or vice versa, which chiefly leads to alimentary metabolism. In the higher forms these relations become more numerous and remote. Now odours, sounds and pictures attract the animals to food substances already in wide regions of the surrounding world. And in the highest formation the sound of speech, as well as written and printed characters, send human beings all over the world in search of daily bread. Thus, numberless, diverse and distant external agents act, as it were, as food signals, directing the higher animals to acquire it and making them establish food connection with the external world. Along with this variety and remoteness, there takes place a substitution of the temporary for the constant connection between the external agents and the organism; first, because, essentially, the remote connections are of a temporary and changeable nature, and, secondly, because, due to their variety and number, they cannot be covered as constant connections, even by the most capacious apparatus. The given food object may be now in one place, now in another; it may, consequently, be accompanied at one time by certain phenomena, at another time by quite different ones; it may be part of one or another system of the external world, and therefore now these now other natural phenomena must temporarily serve as stimulating agents producing in the organism a positive motor (in the broad sense of this word) reaction to this object. In order to make the second proposition more comprehensible—that distant connec-

tions cannot be of a constant nature—I shall make a comparison. Suppose that instead of the present system of telephonic communication effected through the central telephone exchange, that is, temporary communication, all the subscribers were permanently connected with one another. How expensive, inconvenient and indeed impracticable it would be! All that is lost in this case by the conditional nature of the connection (one cannot get connected every moment with every subscriber) is largely compensated by the wide range of possible connections.

How is the temporary connection, the conditioned reflex formed? For this purpose it is necessary that the new indifferent external agent should coincide in time once, or more than once, with the action of the agent already connected with the organism, i.e., which calls forth this or that activity of the organism. Given this coincidence, the new agent enters into the same connection and manifests itself in the same activity as the old one. Thus, a new conditioned reflex is formed with the help of the old one. In the higher nervous system, where the process of formation of conditioned reflexes occurs, the following procedure takes place: if a new, previously indifferent stimulus, upon entering the cerebral hemispheres, meets in the nervous system at that moment a focus of strong excitation, it begins to concentrate, as if working its way to this focus, and thence to the corresponding organ; thus it becomes a stimulus of that organ. On the contrary, when there is no such focus, it disperses in the mass of cerebral hemispheres without producing any pronounced effect. Such, then, is the formulation of the fundamental law of the higher part of the nervous system.

Allow me now, very briefly, to illustrate *with facts* what I have just said about the mechanism of forming conditioned reflexes.

So far all our research has been done exclusively on the small, physiologically insignificant organ—on the salivary gland. This choice, although at first accidental, proved most successful and even fortunate. In the first place, it corre-

sponded to the fundamental requirement of scientific thought, namely, in the field of complex phenomena to begin with the simplest possible case; in the second place, this organ made it possible clearly to distinguish between simple and complex forms of nervous activity, so that they could be easily contrasted. It was this that led to comprehension of the matter. Physiology has known for years that the salivary glands begin to function, i.e., to secrete saliva in the mouth, when food or other stimulating substances are introduced into the oral cavity, and that this correlation is established by means of definite nerves. These nerves receive the stimulation produced by the mechanical and chemical properties of the substances introduced into the mouth, conduct them first to the central nervous system and thence to the salivary gland, causing there the formation of saliva. This is the old reflex, or, in our terminology, the unconditioned reflex, a constant nervous connection, a simple nervous activity which takes place in exactly the same way as in animals having no higher parts of the brain. At the same time everyone, not only physiologists, knows that the relation of the salivary gland to the external world is highly complex; for example, the sight of food or even the thought of it causes secretion of saliva in a hungry man or animal. According to the old terminology, this signified that secretion of saliva is excited also psychically. The higher parts of the brain are necessary for such complex nervous activity.

The analysis of this particular point revealed that at the basis of this complex nervous activity of the salivary gland, of its complicated relation to the external world, lies the mechanism of the temporary connection—the conditioned reflex, which I described in general terms earlier. Our experiments clarified the matter and brought out indisputable facts. Everything in the external world—every sound, picture, and odour—could be brought into temporary connection with the salivary gland and become an agent stimulating the secretion of saliva—the only condition being that it coincided in time with the unconditioned reflex, with the

flow of saliva caused by the substances introduced into the mouth. In short, we were able to produce as many and as varied conditioned reflexes on the salivary gland as we wished.

At present the theory of conditioned reflexes, based on the work of our laboratories alone, constitutes an extensive chapter with a mass of facts and a number of strict rules connecting them. Here is a very general sketch, or, to be more exact, only the headlines of this chapter. First of all there are numerous details relating to the speed of formation of the conditioned reflexes. Then come various kinds of conditioned reflexes and their general properties. Further, since the centre of the conditioned reflexes is located in the higher part of the nervous system, where collision of numberless influences from the external world is always taking place, it is understandable that a never-ending struggle takes place between the various conditioned reflexes, or a selection of them at any given moment. Hence—constant cases of inhibition of these reflexes. Three kinds of inhibition have now been established—simple, extinguishing and conditioned. Taken together they form the group of external inhibition, since they are based on the addition of a collateral external agent to the conditioned stimulus. On the other hand, an already formed conditioned reflex, because of its internal relations alone, is subject to constant fluctuations, even to complete disappearance for brief periods, i.e., is inhibited internally. For example, if even a very old conditioned reflex is repeated several times without being accompanied by the unconditioned reflex, with whose help it was formed, it begins at once gradually but steadily to lose strength and, more or less quickly, is reduced to zero, i.e., if the conditioned reflex, as a signal of the unconditioned, begins to signalize incorrectly, it gradually loses its stimulating effect. This loss of effect occurs not by the destruction of the conditioned reflex, but solely because of its temporary inhibition, since the conditioned reflex thus extinguished is restored of itself after some time. There are

still other cases of internal inhibition. Further experimentation revealed a new important side of the problem. It proved that, in addition to excitation and inhibition of excitation, inhibition of the inhibition is just as frequent, i.e., disinhibition. It is impossible to say which of these three acts is the most important. It should be simply stated that all higher nervous activity, as manifested in the conditioned reflexes, consists of a constant interchange, or to be more precise, equilibration of these three basic processes—excitation, inhibition and disinhibition.

I shall pass now to the second of the above-mentioned basic mechanisms—the mechanism of the analysers.

As stated above, the temporary connection is a necessity when the relation of the animal to the external world becomes complex. But this complexity of relations presupposes ability on the part of the animal organism to decompose the external world into separates. And it is actually the case that every higher animal possesses diverse and most delicate analysers. These are what until now have been known as the sense organs. The physiological teaching of these organs, as implied by their name, consists in large measure of subjective material, i.e., of observation and experimentation with the sensations and ideas of human beings, and is thus deprived of all the extraordinary means and advantages afforded by the objective study and the practically boundless field of experimentation on animals. It is true that this branch of physiology, thanks to the interest and participation of a number of brilliant investigators, is in some respects the most elaborated branch of physiology and contains much data of great scientific significance. But this elaborate research concerns mainly the physical side of the phenomena in the sense organs, for example, the conditions for the formation of clear pictures on the retina of the eye. But in the purely physiological part, i.e., in the study of the conditions and kinds of excitability of the nerve endings in the given sense organ, there is a multitude of unsolved problems. In the psycholo-

gical part, i.e., in the teaching on sensations and ideas resulting from the stimulation of these organs, only elementary facts have been established, despite the skill and keenness displayed by investigators in this field. That which the great Helmholtz implied by the term "unconscious conclusion" evidently corresponds to the mechanism of the conditioned reflex.[49] When, for example, the physiologist becomes convinced that, for the purpose of getting an idea of the actual dimensions of an object, a certain dimension of its image on the retina is required, as well as a certain action of the external and internal muscles of the eye, he is thereby establishing the mechanism of the conditioned reflex. A definite combination of stimuli coming from the retina and ocular muscles, repeatedly coinciding with the tactile stimulus arising from an object of certain size, acts as a signal and becomes a conditioned stimulus produced by the real size of the object. From this point of view, which will hardly be disputed, the principal facts of the psychological part of physiological optics are, physiologically, simply a series of conditioned reflexes, i.e., of elementary facts relating to the complex activity of the eye analyser. Here, in the final analysis, as in all branches of physiology, more, immeasurably more, remains unknown than is known.

An analyser is a complex nervous mechanism which begins with an external receiving apparatus and ends in the brain, either in its lower, or in its higher part; in the latter case it is much more complex. The basic fact of the physiology of the analysers is that each peripheral apparatus is a special transformer of the given external energy into a nervous process. Then there are numerous problems that are far from having been solved or remain wholly unsolved: how is this transformation effected in its last stage? What underlies the analysis? Which part of the activity of the analyser is to be attributed to the construction and process in the peripheral apparatus, and which to the construction and process in the cerebral ending of the analyser? What are the consecutive stages of this analysis from their sim-

plest to the highest forms? And finally, what are the general laws governing this analysis? At present these are questions for purely objective investigation on animals, by the method of conditioned reflexes.

By establishing a temporary connection between the organism and a certain natural phenomenon it is easy to determine the extent to which the given analyser can decompose the external world. For example, it can be revealed without any difficulty and at the same time with great precision that the ear analyser of the dog differentiates the finest timbres, separate small parts of tones, that it not only differentiates, but firmly retains this differentiation (which in man is called "absolute pitch") and is much more susceptible to high-pitch stimulation than man; it reacts to oscillations of 80 to 90 thousand per second, whereas the limit of the human ear is but from 40 to 50 thousand oscillations per second.

In addition, objective investigation reveals the general rules according to which the analysis is effected. The most important of these is the gradualness of the analysis. The given analyser takes part in the conditioned reflex, in the temporary connection, at first, by its more general and gross activity and only afterwards, being gradually differentiated by the conditioned stimulus, does the activity become highly delicate and refined. For example, if a bright figure appears before the animal, the strong illumination acts first as a stimulus and only afterwards is a special stimulus elaborated from the figure itself, etc.

Further, these experiments on animals with conditioned reflexes clearly revealed that differentiation develops as a result of an inhibitory process, as if through a suppression of all other parts of the analyser except the given one. And it is this gradual development of this process that underlies the gradual analysis. That has been proved by many experiments. I shall refer to one convincing example. If the equilibrium between the excitatory and inhibitory processes is broken down in favour of the former by the administra-

tion of stimulants such as caffeine, then the well-elaborated differentiation is immediately and sharply deranged, and in many cases disappears altogether, although temporarily.

Objective study of the analysers also yielded favourable results in experiments with artificially damaged cerebral hemispheres. These experiments disclosed an important and exact fact: the more the cerebral end of the given analyser is damaged, the less delicate is its work; it continues to enter into the conditioned connection as previously, but only through its more general activity. For instance, when the cerebral end of the eye analyser is considerably damaged, one or another intensity of light easily becomes a conditioned stimulus, but separate objects, definite combinations of light and shadow irretrievably lose their specific stimulating effect.

Concluding this exposition of facts relating to the new field of research, I cannot refrain from a brief reference to the peculiarities of this work. The investigator always has the feeling that he is on sure and extremely fertile ground. He is besieged on all sides by questions, and his task is to establish their most expedient and natural order. Notwithstanding the speed of the research it invariably bears a practical character. One who has not tested the facts for himself can scarcely credit how often these, apparently highly complex relations, which, from the psychological point of view, seem truly enigmatic, are subject to clear and successful objective physiological analysis easily verified at all stages by corresponding experiments. Those working in this field are often struck by the incredible power of objective investigation in this new field of highly complex phenomena. I am convinced that extraordinary enthusiasm and a real passion for investigation will grip all who take to this new domain of research.

Thus, in a purely objective way, on the basis of natural science, the laws of complex nervous activity are being elaborated, and the secrets of its mechanisms gradually disclosed. It would be an unjustified claim to assert that the

entire higher nervous activity of the higher animals is confined wholly and solely to the two general mechanisms described above. But this, too, is unimportant. The future of research is always obscure and fraught with surprises. In this case the essential point is that a vast and boundless domain for investigation has now been opened up, based on natural science and guided by fundamental, purely scientific concepts.

These basic concepts of the highly complex activity of the animal organism fully harmonize with the most general picture of it from the standpoint of natural science. As part of nature, each animal organism is a complex and integral system, the internal forces of which, so long as it exists, are equilibrated at every moment with the external forces of the surrounding medium. The more complex the organism, the more delicate, manifold and diverse are the elements of its equilibration. There are analysers and mechanisms both of constant and temporary connections which serve this purpose; they establish the most precise relations between the most minute elements of the external world and the most delicate reactions of the animal organism. Thus, life as a whole, from the simplest to the most complex organisms, including man, of course, is a long series of equilibrations with the environment—equilibrations which reach the highest degree of complexity. And the time will come, distant or not, when mathematical analysis based on natural science will express in majestic formulae of equation all these equilibrations, including, in the final analysis, itself.

But in stating all this, I should like to avoid any misunderstanding in relation to myself. I do not deny psychology as the knowledge of the inner world of the human being. Even less am I inclined to deny anything which concerns the deepest aspirations of the human spirit. Here I now simply uphold and assert the absolute and incontestable right of natural science to operate wherever and whenever it is *able* to display its power. And who knows the limits to this!

In conclusion allow me to say something about the practical side of this new field of research.

The researcher who has resolved to register *all* the influences of the external environment on the animal organism requires exceptional equipment for his investigations. He must have in his hands all the external influences. That is why he needs an absolutely new, hitherto unprecedented, type of laboratory, where there are no accidental sounds, no sudden fluctuations of light, no abruptly changing air draughts, etc.; in short, it must be a laboratory with the maximum evenness, where the investigator has at his disposal the drives of generators producing all kinds of energy, and the widest range of corresponding analysers and measuring instruments. Here, there must be real competition between the modern technique of the physical instruments and the perfection of the animal analysers. This combination will result in a close alliance between physiology and physics, which, it can be assumed, will greatly benefit physics.

At present, because of existing laboratory conditions, the work in question is often not only restricted, contrary to our will, but almost always entails considerable difficulties for the experimenter. He may have spent weeks preparing for his experiment, and at the very last moment, when he is patiently waiting for positive results, a sudden vibration of the building, a noise from the street, etc., destroys his hopes and delays the desired answer indefinitely.

The right kind of laboratory for this investigation is, in itself, of great scientific importance, and since our country has laid the foundations for this kind of research I would like to see it build the first appropriate laboratory so that this, as it seems to me, highly important scientific establishment should redound solely to our honour and credit. This, of course, can be achieved with the help of public interest and initiative. In conclusion I must confess that this speech has been prompted and encouraged predominantly and mainly by the hope that public interest will be shown here, in Moscow, in this home of Russian glory.

"PURE PHYSIOLOGY" OF THE BRAIN[50]

I have been invited by the President of the Organizing Committee of this congress to read a paper before the Psychology Section on the cerebral activity based on the work of the laboratories of which I am in charge. I have readily accepted the invitation since I feel that an exchange of views between representatives of psychology on this vital problem is an urgent necessity.

Some years ago our esteemed president wrote the following words: "When the physiologists succeed in creating alongside psychology a physiology of the brain—I have in mind *pure physiology*, and not the psychological imitation appearing under this name, a physiology capable of speaking for itself, without psychology prompting it word by word what it ought to say—then we shall see whether or not it would be useful to abolish human psychology and, consequently, comparative psychology. But we have not yet reached that stage."[51]

One cannot but admit the justness of this criticism of the situation as it then was, and that the general formulation of the question is most helpful.

Basing myself on the facts acquired over the years jointly with about a hundred colleagues, and also on the facts accumulated by other investigators, I make bold to state with full conviction that physiology of the cerebral hemispheres (and a "real" physiology at that, in the sense of Prof. Claparède) has made its appearance and is rapidly developing; in studying the normal and pathological activ-

ity of the cerebral hemispheres in animals it uses exclusively physiological concepts and has no need whatever to resort to psychological concepts and terminology. Its research rests on the solid foundation of facts, in the same way as the other natural sciences, with the result that exact material is being accumulated at a truly irrepressible rate and the horizon of investigation is constantly widening.

I shall give now only the barest general outline of the fundamental concepts and facts of this physiology of the brain in order to dwell later in detail on one of its points which seems to me particularly appropriate and of special interest for our present meeting.

The basic functions of the higher part of the central nervous system are the coupling of new and temporary connections between the external phenomena and the work of the different organs, and the decomposing by the organism of the complex of the external environment into its separate elements, that is, functions of coupling and analysing mechanisms.

By means of these activities there are established finer and more delicate adjustments of the animal organism to the environment, or, in other words, a more complete equilibration of the system of matter and energy which constitute the animal organism, with the matter and energy of the environment.

The constant connection between certain phenomena and the function of the organs has long been studied by physiologists as the activity of the lower part of the central nervous system and has been called by them reflexes. The function of the higher part of the central nervous system consists in forming new, temporary reflexes; this means that the nervous system is not only a conducting, but a coupling apparatus. Thus, modern physiology distinguishes two kinds of reflexes—constant and temporary (inborn and acquired, reflexes of species and those of the individual). From the purely practical point of view, we call the first reflex unconditioned and the second—conditioned. It is high-

ly probable (and there are indications to this effect) that newly formed reflexes, given the same conditions of life in the course of successive generations, invariably become constant reflexes. Consequently, this must be one of the acting mechanisms in the evolution of the animal organism.

Similarly, elementary analysis is effected by the lower part of the central nervous system, but this, too, like the inborn reflex, has been studied by physiology for a long time already. When, for example, different physiological effects are produced in a decapitated organism by skin stimuli of different quality and location, we have before us the activity of the lower analysing apparatus. In the higher levels of the central nervous system there are the endings of the most delicate and infinitely diverse analysers; the smallest elements of the external world, which are isolated by them, constantly make new connections with the organism and form conditioned reflexes, whereas in the lower parts relatively fewer and more complex agents of the external world participate in the formation of constant reflexes.

As is known, the entire route along which the nervous excitation in an inborn unconditioned reflex travels, is called the reflex arc. Three parts of this arc are rightly distinguished in the lower central nervous system: the receptor (receiving apparatus), the conductor (conducting apparatus) and the effector (apparatus effecting the action). If we add to "receptor" the word "analyser" (the decomposing apparatus) and to "conductor" the word "contactor" (the coupling apparatus), we have a similar anatomical substratum for the two basic functions of the higher part of the central nervous system.

As has long been established by numerous investigators, the conditioned reflex is invariably formed in the presence of a small number of definite conditions; hence, there are no grounds whatever for regarding its formation as an especially complex process. When a certain indifferent stimulus coincides in time with the action of another stimulus producing a definite reflex, after one or several such coinci-

dences this indifferent stimulus itself invariably evokes the same reflex.

In our experiments on dogs we always used two unconditioned reflexes for the elaboration of new conditioned reflexes—the reflex evoked by food and the reflex evoked by introducing acid into the mouth; we measured the secretory reaction of the salivary glands and only occasionally noted motor reactions—positive in the first case and negative in the second. A conditioned reflex can be elaborated in a similar way with the help of an old conditioned reflex. It can be formed also from a stimulus already firmly connected with a certain reflex, even a stable one. Such a conditioned reflex was obtained by us in the case of a destructive stimulus. If the skin of the dog is stimulated by a more or less strong electric current, there is, naturally, a defensive reaction on the part of the animal. By combining this stimulus with repeated feeding of the dog we can make the same current, or even a current of greater strength, as well as any other mechanical or thermal destruction of the skin, produce not a defensive, but a strong alimentary reaction without any signs of the former (the dog turns towards the food and an abundant secretion of saliva begins). A highly essential detail in the elaboration of the conditioned reflex is that the supposed conditioned stimulus should not exactly synchronize with the stimulus of the old reflex, but precede the latter somewhat (by a few seconds).

I shall omit many details relating to the elaboration of conditioned reflexes, their systematization, general characteristics, etc.

As to the activity of the analysers, the first thing to be observed is that in the initial phase all stimuli enter into the new reflex in their general form and only afterwards do they gradually become specialized. If, for example, we elaborate a conditioned stimulus from a given tone, then at first other tones and even other sounds (beats and noises) also produce the same reflex; later, when the conditioned stimulus has been repeated frequently, the range of stimu-

lating sounds becomes smaller and smaller until only the selected tone, and even a certain part of it, evokes the conditioned reflex. In this way the limits of the activity of the analysers are defined; in some analysers of the animal on which we experimented this activity was of incredible delicacy and had possibilities of wide development. A greater or lesser destruction of the brain end of the analyser is respectively reflected in a greater or lesser decline of the degree of analysis.

Again I shall omit many particulars relating to these facts.

Both the conditioned reflex and the process of analysis are subject to constant fluctuations during the normal course of life. I shall not touch now on their chronic changes, but both of them manifest rapid variations, now stronger, now weaker. Up to the present time we have studied most thoroughly the phenomenon of rapid diminution of the activity of the conditioned reflexes. The term "inhibition," generally accepted in physiology, is used by us to denote this phenomenon; we have all the grounds for distinguishing three kinds of this inhibition: external, internal and sleep inhibition.

External inhibition fully reproduces the inhibition which physiology long ago recognized in the lower part of the central nervous system, when the new additional reflex inhibits the one already existing and active. Evidently, this is the expression of constant, non-stop competition between different external and internal stimulations for a relatively predominant role in the organism at the given moment. External inhibition, in its turn, is subdivided into several types.

Internal inhibition has its origin in the mutual relations between the new reflex and the old one with the help of which it was formed; it always develops when the conditioned reflex temporarily or constantly (in the latter case only under a definite new condition) is not accompanied by the stimulus with the aid of which it was elaborated. So

far we have studied four kinds of such inhibition. For the sake of brevity I shall dwell only on one of them, the earliest investigated by us. This is the so-called extinction of the conditioned reflex. If an elaborated conditioned stimulus is repeated several times at definite short intervals (two, three, five minutes and more) without being accompanied by the old stimulus with the help of which it was formed, then it gradually weakens and, finally, becomes wholly ineffective. This, however, does not signify destruction of the conditioned reflex, but only its temporary inhibition, since after some time it is completely restored in a spontaneous way. I would like you to keep in mind this kind of internal inhibition since I shall revert to it later in connection with the most important point in my paper.

All kinds of internal inhibition may be disturbed, suppressed and, so to speak, inhibited themselves, i.e., the reflexes inhibited by them become liberated, disinhibited, if external inhibiting agents of moderate strength act on the animal. That is why the study of the phenomena of internal inhibition calls for a specially equipped laboratory, otherwise all accidental agents, and more frequently, of course, acoustic phenomena, may constantly interfere with the experiments.

Finally, the last kind of inhibition is the sleep inhibition, which regulates the proper chemical metabolism of the entire organism, and especially the nervous system. It assumes the form of normal sleep or of hypnotic state.

When describing the nervous activity it is necessary always to take into account the absolute and relative strength of the various stimuli and the duration of their latent traces. Both phenomena clearly manifest themselves in the course of our experiments and can be studied and measured without difficulty. Moreover, one can say that here the most striking phenomenon is the predominance of the law of force and measure; and involuntarily the thought comes to mind that it is not at all accidental that mathematics—the

teaching on the relations of numbers—had its origin wholly and solely in the human brain.

The separate features of the nervous systems of different animals were manifested in our experiments with particular force, and can be expressed in exact figures. An example will be given below.

In the course of our investigation of the two basic cerebral functions we gradually disclosed the fundamental properties of the brain mass. One of these is a peculiar movement of the nervous processes in this mass. On the basis of our latest experiments I am in a position to submit to you now, in truly striking form, the fundamental law of the higher nervous activity. This is the law of irradiation and subsequent concentration of the nervous process. This law applies both to excitation and inhibition. It has been frequently and with particular thoroughness investigated by us in the phenomena of internal inhibition. I take the liberty of directing your attention to these experiments.

Before us is a dog, on which by means of the action of acid in the oral cavity as an unconditioned stimulus, a mechanical irritation of more than twenty places on the skin has been made the conditioned stimulus of the acid reaction, i.e., mechanical irritation of these places (effected by a special device) evokes, each time, secretion of a definite quantity of saliva and a corresponding motor reaction. The effect obtained from the stimulation of any of these places on the skin is equal. Now for the experiment itself. Let us apply the mechanical irritation to a certain point of the skin for a definite period, say thirty seconds. We obtain a salivary reflex which is strictly measurable in certain units. This time, to the conditioned stimulus we do not add the introduction of acid into the mouth as an unconditioned stimulus, and after a certain interval, say two minutes, we repeat the application of the conditioned stimulus. In this way we get a decreased reflex. We continue to repeat the application of the conditioned stimulus until the conditioned reflex is reduced to zero. This is what we have

termed the extinction of the conditioned reflex, one of the kinds of internal inhibition. We have thus evoked the process of inhibition in a certain point of the brain end of the cutaneous analyser, i.e., in the area of the cerebral hemispheres connected with the skin. Let us now follow the development of this process. Immediately after obtaining the zero effect on the repeatedly stimulated point of the skin (primary extinction) we begin to stimulate, without interruption, a new point 20 or 30 cm. distant from the first (our dog being of average size). We obtain here a normal effect equal to, say, thirty divisions on the tube with which we measure the quantity of the secreted saliva. We repeat this experiment (in one day, in two days, etc.) in the following way: we stimulate a new, distant part of the skin not immediately after obtaining the zero effect on the place of primary extinction, but five seconds later. Now the secretion of saliva is reduced to, say, twenty divisions (secondary extinction). With the next repetition of the experiment, but after an interval of fifteen seconds, the secretory effect is reduced to five divisions. After an interval of twenty seconds it falls to zero. Let us continue. After an interval of thirty seconds, a secretory effect reappears equalling three or five divisions. After an interval of forty seconds we get from fifteen to twenty divisions, after fifty seconds—from twenty to twenty-five divisions, and after sixty seconds—the customary effect is fully restored. Throughout this period (sixty seconds and even much longer) all attempts to irritate the point of the primary extinction have no effect whatsoever. We obtain the same series of figures, no matter which two points of the skin we choose for primary and secondary extinction, provided the distance between them remains the same. If the distance between the stimulated points is decreased, then the difference is as follows: the decrease of the secretory effect and the zero effect at the point of the secondary extinction appear earlier, the zero effect persists longer and the return to normal takes place later. These experiments, provided, of course, that all the

necessary precautions are observed, proceed with marvellous exactitude. These experiments, carried out by two experimenters on five dogs, were observed by me in the course of one year. Their stereotype character was astonishing, and I can say without exaggeration that for a long time I could not believe my eyes.

If we compare these facts with other similar facts and exclude various other hypotheses, we arrive at the following natural and simple conclusion. Regarding the skin as the projection of a definite area of the brain, we must assume that the process of internal inhibition arising in a certain point of this area first spreads, irradiates over the entire area, and immediately afterwards begins to concentrate around the point of origin. It is worth noting that this movement in both directions proceeds very slowly. Also of interest is the fact that this speed, which greatly varies in different animals (in the relation of 1 : 5 and even more) remains for any given animal highly stable, one might even say, invariable.

As we see, the law of irradiation and concentration of the nervous process is of great importance. It can establish the relation between many, seemingly quite different, phenomena, for instance, the generalized character of each individual stimulus when it first becomes a conditioned stimulus, the mechanism of external inhibition; and the formation of the conditioned reflex itself, which can be understood as a phenomenon of concentrated stimulation. However, I shall not now go into detailed consideration of the significance of this law; I shall simply avail myself of the foregoing experiment, which illustrates this law, for some special purpose.

During thirteen years' work jointly with my colleagues on conditioned reflexes, I have always had the impression that the psychological concepts and the systematization of subjective phenomena by the psychologists must profoundly differ from physiological concepts and physiological classification of the phenomena of the higher nervous activity;

that the reproduction of the nervous processes in the subjective world is very peculiar, is, so to speak, many times refracted, with the result that the entire psychological concept of the nervous activity is highly conventional and approximate. And it is from this point of view that the above-mentioned experiment deserves special attention.

When we first established the fact of extinction of the conditioned reflex, many people used to say: "There is nothing unusual in this. The explanation is quite simple. The dog notices that the signal no longer corresponds to reality and, therefore, begins to react more and more weakly, until, finally, there is no reaction at all."

I believe that many of you who uphold the scientific validity of zoopsychology would say the same thing. Be that as it may. But then, gentlemen, it seems to me that you are obliged to interpret psychologically the experiment described above in detail and in all its phases. I have suggested this many times to men of different specialities (naturalists and sociologists). The result was most definite: each gave his own interpretation, i.e., fancied in his own way one or another internal state of the animal; however, it proved impossible in most cases to harmonize or reconcile their explanations. The zoopsychologists spoke of the ability of the animal to make distinctions, to remember things, to draw conclusions, to experience confusion, disappointment, and other similar qualities in a variety of combinations. In reality there took place in the nervous mass only irradiation and subsequent concentration of the inhibitory process, and this knowledge made possible an absolutely exact prediction (in figures) of the phenomena.

What can you say in reply, gentlemen? I await your answer with the greatest interest.

Here I conclude the part of my paper which deals with facts. Allow me to make a few supplementary remarks. All parts of the higher nervous activity of the experimental animals are gradually involved in our investigation of the conditioned reflexes; one can see this even from a rough,

approximate comparison between the external facts under our observation, and the psychological classification of such subjective phenomena, as consciousness, thought, will, affect, etc. The meaning of some of these facts became clear to us in the course of our objective study of animals with damaged cerebral hemispheres. Finally, the general condition of the brain, in its active state and at rest, was revealed more and more clearly.

So far the entire field of research opening before us comprises our concept of the two basic activities of the cerebrum—the coupling and the analysing functions—and a few fundamental properties of the brain mass. Life will show whether this is sufficient, since, naturally, our general knowledge of the brain and also of its general properties is bound to be extended and deepened.

And so, as mentioned above, the horizon of strictly objective investigation of the higher nervous activity is steadily widening. Why, then, should physiology strive to penetrate into the hypothetical fantastic internal world of the animal? In thirteen years of research I have never had success with psychological concepts. The physiology of the animal brain must not for a single moment leave the ground of natural science, which every day proves its absolute solidity and extreme fruitfulness. We can rest assured that along the path taken by the strict physiology of the animal brain, astonishing discoveries await science and, together with them, extraordinary power over the higher nervous system—discoveries and power not a whit inferior to other achievements of natural science.

I greatly appreciate the contribution of the old and the new psychologists, but it seems to me, and it can hardly be doubted, that this work is being done in an extremely inefficient way, and I am fully convinced that the pure physiology of the animal brain will greatly facilitate, and, moreover, enrich the Herculean labours of those who have devoted their lives to the study of the subjective states of man.

RELATION BETWEEN EXCITATION AND INHIBITION, DELIMITATION BETWEEN EXCITATION AND INHIBITION, EXPERIMENTAL NEUROSES IN DOGS[52]

Dedicated to the memory of my best friend, Professor Robert Tigerstedt, to whom physiology owes so much for his investigations and for his work in promoting physiological knowledge and physiological research.

All the factual material which follows relates to the work of the cerebral hemispheres and has been obtained by the method of conditioned reflexes, i.e., reflexes formed in the course of the animal's individual life. Since the concept of conditioned reflexes is not yet generally known and recognized among physiologists I shall, for the purpose of avoiding repetition, refer the reader to my articles recently published in these archives[53] (1923).

Proceeding from the big difference between the phenomena, we had to distinguish two kinds of inhibition in the work of the cerebral hemispheres—external and internal—according to our terminology. The former appears in our conditioned reflexes at once; the latter develops with the passage of time and is elaborated gradually. The first is an exact repetition of the well-known inhibition in the physiology of the lower part of the central nervous system, which appears when stimuli acting on the various centres and evoking different nervous activities, meet; the second can be inherent only in the cerebral hemispheres. It may be,

however, that the difference between these kinds of inhibition is connected only with the conditions of their emergence and not with the essence of the process itself. This question is still being investigated by us. The present article deals only with internal inhibition; further, I shall call it simpy inhibition, without the adjective, although each time implying internal inhibition.

There are two conditions, or to be more precise, one condition, the presence or absence of which determines whether the impulse brought into the cells of the cerebral hemispheres from the outside chronically provokes a process of excitation or a process of inhibition. In other words, the impulse will in one case become positive and in the other negative. This fundamental condition consists in the following: if the stimulation coming to a cerebral cell coincides with another extensive stimulation of the cerebral hemispheres, or of a definite lower part of the brain. then it will always remain positive; given the reverse condition it will, sooner or later, become a negative, inhibitory stimulus. Of course this indubitable fact gives rise to the question: why is this so? But so far there has been no answer to this question. Thus, we must proceed from this fact without having analysed it. Such is the first basic relation between excitation and inhibition.

Physiologists have long been aware of the irradiation of the excitatory process. The study of the higher nervous activity led us to the conclusion that the inhibitory process, too, spreads, under certain conditions, from the point where it is originated. The facts underlying this conclusion are perfectly plain and obvious. Now, if the excitatory process spreads from one point, and the inhibitory process from another, they limit each other and confine each other to a definite area and within definite bounds. In this way a very delicate functional delimitation of separate points of the cerebral hemispheres can be obtained. When these separate points are subjected to excitation under corresponding conditions, it can be easily explained by the scheme of the cellular con-

struction. But this interpretation meets with certain difficulties when there is an excitatory or inhibitory process related to various intensities or other similar variations (for example, to different frequencies of the metronome beats) of one and the same elementary external stimulating agent. In order to explain this on the basis of the same simple cellular scheme, it would be necessary to assume as a point of application of this agent not a single cell but a group of cells. In any case, it is actually possible to associate the excitatory process with one intensity of a certain elementary agent and the inhibitory process with another. Thus, the second general relation between excitation and inhibition consists in their mutual spatial limitation, in their delimitation. A clear demonstration of this is obtained by the experiments with mechanical stimulation of various points of the surface of the skin.

Thus, we have to assume that a certain conflict takes place between two opposing processes which normally ends in the establishment of a definite equilibrium between them, in a definite balance. This struggle and this equilibration confront the nervous system with a difficult task. We have seen this from the very outset of our research, and we are seeing it now. This difficulty is often manifested in the animal in the form of motor excitation, whining and dyspnoea. But in most cases equilibrium finally sets in; each process is allotted its place and time, and the animal becomes perfectly quiet, reacting to respective stimuli now by the excitatory, now by the inhibitory process.

Only under certain conditions does this conflict end in disturbance of the normal nervous activity; then a pathological state sets in which lasts for days, weeks, months and perhaps even years, and either gradually returns to the normal of itself after the experiments have been discontinued for a time and the animal has been allowed rest, or it must be eliminated by definite treatment.

These special cases at first emerged spontaneously, unexpectedly, but later they were deliberately produced by us

for research purposes. We describe them here in chronological order.

The first of these cases was obtained by us a long time ago (experiments of Dr. Yerofeeva). It consisted in the following. The conditioned alimentary reflex was elaborated in the dog not from an indifferent agent, but from a destructive one, provoking an inborn defensive reflex. The animal's skin was irritated by an electric current, and at the same time the animal was fed, at first even forcibly. In the initial phase a weak current was applied, but later it was increased to the maximum. The experiment ended thus: the strongest current, as well as the severe burning and mechanical destruction of the skin, provoked only an alimentary reaction (a corresponding motor reaction and a secretion of saliva) without any sign of a defensive reaction, or even of any change in respiration and heartbeat—the usual accompaniments of this reaction. Evidently this result was obtained by transferring the external excitation to the food centre and simultaneous inhibition of the centre of the defensive reaction. This specific conditioned reflex persisted for months, and probably would have remained unchanged under the given conditions had we not begun to modify it, systematically transferring the electric irritation to new points of the skin. When the number of these points became considerable, the picture suddenly and abruptly changed in one of our dogs. Now only a very strong defensive reaction manifested itself everywhere, even in the first location of the skin stimulus and under the action of the weakest current; there was no trace of the alimentary reaction.

The old result could not be reproduced. The dog which had previously been quiet became greatly excited. In another dog a similar result was obtained only when—notwithstanding the large number of points on the skin from which we could produce only an alimentary reaction under the application of a strong current—we frequently and quickly, in the course of one and the same experiment, trans-

ferred the irritation from one place to another. We had to allow rest to the dogs for several months, and only in one of them were we able, acting slowly and cautiously, to restore the conditioned alimentary reflex to the destructive agent.

The second case of a similar character was observed somewhat later (experiment of Dr. N. R. Shenger-Krestovnikova). A conditioned alimentary reflex was brought about in a dog by a circle of light projected on a screen placed in front of the animal. We then began to elaborate a differentiation of the circle from an ellipse of the same size and intensity of light, i.e., the appearance of the circle was accompanied each time by feeding, whereas that of the ellipse was not. In this way the differentiation was obtained. The circle evoked an alimentary reaction, but the ellipse remained ineffective, which, as we know, is a result of development of inhibition. The ellipse which was applied first greatly differed in form from the circle (the proportion of its axes was 2 : 1). Then the form of the ellipse was brought closer and closer to that of the circle, i.e., the axes of the ellipse were gradually equalized, and thus sooner or later we were able to obtain an increasingly delicate differentiation. But when we applied an ellipse whose axes were as 9 : 8, the picture abruptly changed. The new delicate differentiation, which always remained incomplete, persisted for two or three weeks, after which it not only disappeared itself, but caused the loss of all earlier, even the least delicate, differentiations. The dog, which previously behaved quietly in the stand, was now constantly moving about and whining. All differentiations had to be elaborated anew, and the crudest one now demanded much more time than at first. When the final differentiation was reached, the same story was repeated—all the differentiations vanished, and the dog again became excited.

Some time after these observations and experiments we set ourselves the task of investigating this phenomenon more systematically and in more detail (experiments of

Dr. M. K. Petrova). Since it was possible to conclude from the above-mentioned facts that the derangement of normal relations was caused by a difficult collision between the excitatory and inhibitory processes, we carried out on two dogs of different types—one very lively and the other inactive and quiet—experiments first of all with various inhibitors and their combinations. Together with the conditioned reflexes, delayed for three minutes, i.e., when the unconditioned stimulus was added to the conditioned only three minutes after the beginning of the latter, owing to which the positive effect of the conditioned reflex appeared only after a preliminary inhibitory period of one or two minutes, other kinds of inhibition were applied (differentiation, etc.). But this task was accomplished by the different nervous systems without any derangement of the normal relations, although with a different degree of difficulty. Then we added the alimentary reflex formed by means of a destructive agent. Now it was sufficient, having evoked this reflex, to repeat it for a certain period of time even on one and the same part of the skin, in order to obtain an acute pathological state. This deviation from the normal occurred in the two dogs in opposite directions. In the lively dog the elaborated inhibitions either suffered to a considerable degree or wholly disappeared and turned into positive agents; in the quiet dog it was the positive salivary conditioned reflexes that either weakened or completely vanished. And these states persisted for months without any spontaneous change. In the lively dog with the weakened inhibitory process a quick and lasting return to the normal was obtained in a few days by means of rectal injections of potassium bromide. It is worth noting that with the appearance of normal inhibition the strength of the positive conditioned action, far from decreasing, was even somewhat increased; consequently, on the basis of this experiment we can assume that the action of bromide does not consist in diminution of nervous excitability, but in regulating nervous activity. In another dog permanent and more or less considerable sali-

vary reflexes could not be restored despite the different means applied for this purpose.

Shortly after these experiments similar results, and even with more instructive details, were obtained with a dog subjected to experimental investigation for quite a different purpose (experiments of Dr. I. P. Razenkov). Many positive conditioned reflexes were elaborated on the animal from various receptors, or several reflexes from one and the same receptor by a certain stimulating agent of varying intensity. Among others there was obtained a reflex to a definite frequency of mechanical stimulation of a certain point on the skin. We then began to elaborate a differentiation from the same place on the skin by means of a mechanical stimulation of another frequency. This differentiation was also obtained without difficulty, and no change in nervous activity was observed. But when, after application of a completely inhibited rhythm of mechanical skin stimulation, we tried without any interval to effect stimulation by a positively-acting rhythm, a peculiar disturbance was manifested in the dog, lasting for five weeks and only gradually ending in a return to the normal, perhaps somewhat accelerated by our special measures. A few days after the collision of the nervous processes occurred, all the positive conditioned reflexes disappeared. This lasted for ten days, after which the reflexes began to reappear, but in a peculiar way: contrary to normal, the strong stimuli remained ineffective or produced the minimum effect; considerable effect was shown only by the weak stimuli. This state persisted for fourteen days and was again superseded by a peculiar phase. Now all the stimuli acted equally, approximately, with the same force as strong stimuli under normal conditions. This lasted seven days, and then came the last period before the return to the normal; this phase was characterized by the fact that the stimuli of average strength greatly exceeded those in the normal state, the strong stimuli became somewhat weaker than in the normal and the weak stimuli lost their action altogether. This, too, lasted for seven days, and then,

finally, came the return to the normal. Repetition of the same procedure which was responsible for the disturbance described above, i.e., repetition of direct, without any interval, transition from the inhibitory mechanical stimulation of the skin to the positively-acting stimulation, resulted in the same disturbance with the same variation in phases, but of considerably shorter duration. With further repetition the disturbance became more and more fleeting, until the same procedure no longer evoked any derangement. The decline of the pathological disturbance was manifested not only in the shortened duration of the abnormal state, but also in a reduction in the number of phases, and in the disappearance of the more abnormal phases.

Thus, the difficult collision between the excitatory and inhibitory processes leads now to a predominance of the excitatory process disturbing the inhibition, or, one may say, to a prolonged increase of the tonus of the excitation, and now to a predominance of the inhibitory process, with its preliminary phases, disturbing the excitation, and increasing the tonus of the inhibition.

But then we witnessed the same phenomena also under other conditions, besides those mentioned above.

Under the action of extraordinary, directly inhibiting stimuli on the animal a chronic predominance of inhibition takes place. This manifested itself with particular force in a number of dogs after the unusual flood that occurred in Leningrad on September 23, 1924, when our experimental animals were rescued with great difficulty and under exceptional conditions. The conditioned reflexes disappeared for some time and only slowly reappeared. For a considerable period after rehabilitation any more or less strong stimulus, which earlier would have been regarded as a very strong conditioned stimulus, as well as the application of a previously elaborated and thoroughy concentrated inhibition, again provoked this chronic state of inhibition either in the form of complete inhibition or of its above-mentioned preliminary phases (experiment of Dr. A. D. Speran-

sky and Dr. V. V. Rickman). To a lesser degree and for a shorter time the same thing is often observed in more normal conditions, such as transferring the animals to a new environment, to a new experimenter, etc.

On the other hand, a slight change in the application of a well-elaborated positive conditioned reflex, namely, an unconditioned stimulus administered directly, without any interval, after the conditioned stimulus, increases the tonus of the excitation to such a degree that the elaborated inhibitions, now under investigation, either fully disappear, or greatly lose in constancy and regularity. And often a frequent interchange of positive and inhibitory reflexes brings the dogs, especially the lively ones, to the highest pitch of general excitation (experiments of Dr. M. K. Petrova and Dr. E. M. Kreps).

However, what has been said above does not exhaust all our facts concerning the relation between excitation and inhibition. In the course of our work we encountered other peculiar cases of the same kind.

We frequently noticed that a distortion of the action of conditioned stimuli took place in certain phases of drowsiness in normal animals.

The positive stimuli lost their effect, while the negative inhibitory ones assumed a positive character (for example, in the experiments carried out by Dr. A. A. Shishlo). In the light of this relation we can explain the frequently recurring fact that in the drowsy state of the animal there begins as it were a voluntary secretion of saliva not observed in the waking state. The explanation is that at the beginning of the elaboration of the conditioned reflexes in a given animal the entire mass of accessory stimuli, one can say, the entire laboratory surroundings, enter into conditioned connection with the food centre, but later all these stimuli become inhibited owing to the specialization of the conditioned stimulus applied by us. It can be assumed that in a state of drowsiness these inhibited agents temporarily recover their original effect.

The temporary transformation of the elaborated inhibitory stimulus into a positive one is also observed in pathological states of the cerebral cortex in intervals between the convulsive fits caused by post-operative cicatrization in the cortex. It is interesting to note that along with this elaborated inhibitory stimulus, only the weakest of all the positive conditioned stimuli, viz., light, acts, also positively, during this time, whereas all other moderate and strong positive conditioned stimuli remain ineffective (experiments of Dr. I. P. Razenkov).

Related to this is the fact, frequently reproduced by us, that accessory stimuli evoking certain reflexes of moderate strength transform in the course of their action the inhibitory reflexes into positive ones (we call it disinhibition).

On the contrary, during disturbance of the cortex, caused by extirpation, the positive conditioned stimuli belonging to the disturbed part of the cortex become inhibitory, a point mentioned in my last article on sleep. This phenomenon is particularly manifest and has been best studied in the cutaneous region of the cerebral hemispheres. (Earlier experiments of Dr. N. I. Krasnogorsky and recent experiments of Dr. I. P. Razenkov.) If the lesion is insignificant the effect produced by the previous positive conditioned mechanical stimulation of the skin is less than normal, and if repeated during one and the same experiment soon becomes inhibitory; being added to other effective stimuli it weakens their effect and when applied alone induces a state of sleepiness in the animal. If the lesion is more severe, it does not, in normal conditions, produce any positive effect, being of a purely inhibitory nature; its application leads to the disappearance of all positive conditioned reflexes in the other parts of the cerebral hemispheres.

But this agent, now inhibitory, may, in certain circumstances, manifest a positive effect. If the animal becomes sleepy of itself, this stimulus, as well as the elaborated inhibitory agent, as mentioned above, produces a slight positive effect. But afterwards this effect can be obtained by

other methods. If we repeatedly apply this stimulus several times with a brief intermission, for example, of five seconds instead of the usual thirty (i.e., if the unconditioned stimulus is added five seconds instead of thirty seconds after the beginning of the conditioned stimulus), then, upon delaying it again for thirty seconds, we may obtain a positive effect, although a fleeting one. Setting in very soon after the beginning of the stimulation, it quickly diminishes in the course of stimulation and finally disappears altogether (pure excitatory weakness). A similar transitory effect can be obtained by means of a preliminary injection of caffeine and by other measures (experiments of Dr. I. P. Razenkov).

Of a somewhat different character, but still related to our subject, are the following facts. Given a very weak general excitability of the cortex, as observed in aged animals (Dr. L. A. Andreyev's experiments) or in animals with removed thyroid glands (experiments of Dr. A. V. Valkov), as well as in certain states brought on in the animals by convulsions during post-operative scarring in the cortex (experiments of Dr. I. P. Razenkov), the inhibitory process either becomes impossible or is greatly weakened.

In such cases only an increase of the tonus of cortical excitability, achieved by application of stronger unconditioned stimuli, can sometimes provoke an inhibitory process.

The phenomenon of reciprocal induction, mentioned by me in the previous, above-mentioned articles (experiments of D. S. Fursikov, V. V. Stroganov, E. M. Kreps, M. P. Kalmykov, I. R. Prorokov and others), is also related to our subject. Finally, the last fact: if separate points of the cortex are reinforced for a prolonged period by a corresponding procedure, some of them as points of excitation and others as points of inhibition, they become highly resistant to attacks, to the influence exerted by opposite processes, and at times call for exceptional measures in order to change their functions (experiments of Dr. B. N. Bierman and Dr. Y. P. Frolov).

All the foregoing facts allow us, it seems to me, to systematize the states to which the cortex is subjected under different influences in a definite consecutive order. At one pole there is the state of excitation, an exceptional increase of the tonus of excitation, when an inhibitory process becomes impossible or is greatly impeded. Next comes the normal, wakeful state, the state of equilibrium between the excitatory and inhibitory processes. This is followed by a long, but also consecutive, series of states transitory to inhibition; the most typical of these are: the equalization state when in contrast to the wakeful state all stimuli, irrespective of their intensity, act with an absolutely equal force; the paradoxical state, when only the weak stimuli act, or when the strong stimuli act, too, but produce a barely noticeable effect; and finally, the ultra-paradoxical state when only the previously elaborated inhibitory agents produce a positive effect—a state followed by complete inhibition. There is yet no clear explanation of the state when excitability is so low that inhibition is utterly impossible or greatly impeded, just as in the case of the state of excitation.

At present, among other things, we are engaged in the experimental solution of the following question (for which we now have some clues): are there not in evidence the transitory states so sharply expressed in pathological cases also in all cases of normal transition from an active state to a state of inhibition, such as the process of falling asleep, the process of elaborating inhibitory reflexes, etc.?

Should this be so, then only the retardation, certain isolation and fixation of the states which normally develop and change quickly, or almost imperceptibly, bear a pathological character.

The above facts open the way to an understanding of numerous phenomena relating both to the normal and pathological higher nervous activity. I shall give some examples.

I have already shown in previous articles how normal behaviour is based on the elaborated delimitation of the

points of excitation and inhibition, on their grandiose mosaic in the cortex, and how sleep represents irradiated inhibition. We are now in a position to give some details showing how certain variations of normal sleep, as well as separate symptoms of the hypnotic state, can be easily understood when regarded as different degrees of extensiveness and intensiveness of the inhibitory process.

Cases of sleep setting in while walking or riding horseback are not unknown. This means that the inhibition is confined only to the cerebral hemispheres and does not spread to the lower centres established by Magnus.[54] We know also of sleep accompanied by partial wakefulness in relation to definite stimuli, for instance, the sleep of the miller who wakes when the noise of the mill stops, the sleep of the mother awakening at the faintest sound coming from her sick child, but who is not disturbed by other and much stronger stimuli, i. e., in general a sleep with easily excitable points on guard. Catalepsy in hypnosis is, apparently, an isolated inhibition only of the motor region of the cortex, not affecting all the other parts of the cortex and not spreading to the centres of equilibrium of the body. Suggestion in hypnosis can be rightly interpreted as such a phase of inhibition when weak conditioned stimuli (words) produce a greater effect, evidently, than the stronger direct and real external stimuli. The symptom established by Pierre Janet[55]—loss of the sense of reality during sleep lasting for many years, can be explained as chronic inhibition of the cortex which is interrupted only for a short time and only under weak stimuli (usually at night); this inhibition particularly concerns the cutaneous and motor regions which are most important for the influence of the external world on the organism, on the one hand, and for the real action of the organism on the external world, on the other. Senile talkativeness and dementia are easily explained by the extreme weakening of inhibition in cases of very low excitability of the cortex. Finally, our experiments on dogs entitle us to regard chronic deviations of the higher

nervous activity from the normal, produced in the animals by us, as pure neurosis; to a degree they also explain the mechanism of the origin of these deviations. Similarly the action of exceedingly strong, extraordinary stimuli (for example, unusual flood) on dogs with a weak nervous system and a predominance of the inhibitory process under normal conditions, in other words, with a constantly increased tonus of inhibition, reproduces the aetiology of a special traumatic neurosis.

As for a theory that would cover and generally substantiate all these phenomena, it is obvious that the time has not yet come for it, although many hypotheses have been advanced, each one of them justified to a degree. It seems to me that as things are at present it is possible to make use of the different concepts which actually systematize the factual material and advance new and detailed problems. In our experiments so far we think of different phases, from extreme excitation to deep inhibition, which develop in the nervous cells of the cortex under the influence of effective stimuli, and which depend on the intensity and duration of these stimuli and on the conditions under which the latter are formed. We incline to this view because of the obvious analogy between the changes observed in the activity of the cerebral cortex and the changes taking place in the nerve fibre under various strong influences, which have been described in the well-known work of N. E. Wedensky—*Excitation, Inhibition and Narcosis*.[56] We do not share his theory, but we have grounds for relating all the observed transitions from excitation to inhibition to one and the same elements—to the nerve cells—just as Wedensky rightly did in the case of the nerve fibre.

One can hardly doubt that only the study of the physicochemical process taking place in the nerve fibre will provide us with a real theory of all nervous phenomena, and that the phases of this process will give us an exhaustive explanation of all external manifestations of the nervous activity, of their sequence and interconnections.

THE CONDITIONED REFLEX[57]

The conditioned reflex is now used as a separate physiological term to denote a certain nervous phenomenon, the detailed study of which has led to the creation of a new branch in the physiology of animals—the physiology of the higher nervous activity, as the first chapter in the physiology of the higher parts of the central nervous system. For many years empirical and scientific observations have been accumulated which show that a mechanical lesion or a disease of the brain, and especially of the cerebral hemispheres, causes a disturbance in the higher, most complex behaviour of the animal and man, usually referred to as psychical activity. At present hardly anyone with a medical education would doubt that our neuroses and psychoses are connected with the weakening or disappearance of the normal physiological properties of the brain, or with its greater or lesser destruction. But the following persistent, fundamental questions arise: what is the connection between the brain and the higher activity of the animal and man? With what and how must we begin the study of this activity? It would seem that psychical activity is the result of the physiological activity of a certain mass of the brain and that physiology should investigate it in exactly the same way as the activity of all other parts of the organism is now being successfully investigated. However, this has not been done for a long time. Psychical activity has long (for thousands of years) been the object of study by a special branch of science—psychology. But phys-

iology, strange as it may seem, only recently—in 1870—obtained with the help of its usual method of artificial stimulation the first precise facts relating to a certain (motor) physiological function of the cerebral hemispheres; with the help of its other usual method of partial destruction it acquired additional facts relating to the establishment of connections between other parts of the cerebral hemispheres and the most important receptors of the organism—the eye, the ear, etc. This raised hopes among physiologists, as well as psychologists, that close connection would be established between physiology and psychology. On the one hand, the psychologists used to begin text-books on psychology with a preliminary exposition of the theory of the central nervous system, and especially of the cerebral hemispheres (sense organs). On the other hand, the physiologists when experimenting with the destruction of various parts of the hemispheres in animals viewed the results obtained by them psychologically, by analogy with the human internal world (for example, Munk's assertion that the animal "sees," but "does not understand").[58] However, both camps soon became disappointed. The physiology of the cerebral hemispheres perceptibly stopped at these first experiments and made no further substantial advance. In the meantime many resolute psychologists again took up the cudgels saying that psychological research should be fully independent of physiological. At the same time there were other attempts to link the triumphant natural science with psychology through the method of numerical measurement of psychical phenomena. At one time an attempt was made to create in physiology a special branch of psychophysics on the basis of the fortunate discovery by Weber and Fechner of the law[59] (named after them) which establishes a certain numerical relation between the intensity of an external stimulus and the strength of a sensation. But the new branch failed to go beyond this single law. More successful was the attempt made by Wundt,[60] a physiologist who became a psychologist and philosopher, experimentally to apply

the method of numerical measurement to psychical phenomena in the form of the so-called experimental psychology; thus, considerable material has been collected already and more is being accumulated. Mathematical analysis of the numerical material obtained by experimental psychology is called by some people, as Fechner did it, psychophysics. But now even among psychologists and especially psychiatrists, there are many who are bitterly disappointed in the practical application of experimental psychology.

So what is to be done? However, a new method of solving the fundamental question was already on the way. Was it possible to discover an elementary psychical phenomenon which at the same time could be fully and rightly regarded as a purely physiological phenomenon? Was it possible to begin with it, and by a strictly objective study (as generally done in physiology) of the conditions of its emergence, its various complexities and its disappearance, to obtain first of all an objective physiological picture of the entire higher nervous activity in animals, i.e., the normal functioning of the higher part of the brain, instead of the previous experiments involving its artificial irritation and destruction? Fortunately, such a phenomenon had long been observed by a number of researchers; many of them paid attention to it and some even began to study it (special mention should be made of Thorndike[61]), but for some reason or other they stopped the study at the very beginning and did not utilize the knowledge of this phenomenon for the purpose of elaborating a fundamental method of systematic physiological study of the higher activity in the animal organism. This was the phenomenon now termed the "conditioned reflex," thorough study of which has fully justified the previously expressed hope. I shall mention two simple experiments that can be successfully performed by all. We introduce into the mouth of a dog a moderate solution of some acid; the acid produces a usual defensive reaction in the animal: by vigorous movements of the mouth it ejects the solution, and at the same time an abundant

quantity of saliva begins to flow first into the mouth and then overflows, diluting the acid and cleansing the mucous membrane of the oral cavity. Now let us turn to the second experiment. Just prior to introducing the same solution into the dog's mouth we repeatedly act on the animal by a certain external agent, say, a definite sound. What happens then? It suffices simply to repeat the sound, and the same reaction is fully reproduced—the same movements of the mouth and the same secretion of saliva.

Both of the above-mentioned facts are equally exact and constant. And both must be designated by one and the same physiological term—"reflex." Both disappear if we sever either the motor nerves of the mouth musculature and the secretory nerves of the salivary glands, i.e., the efferent drives, or the afferent drives going from the mucous membrane of the mouth and from the ear, and finally, if we destroy the central exchange where the nervous current (i.e., the moving process of nervous excitation) passes from the afferent to the efferent drives; for the first reflex this is the medulla oblongata, for the second it is the cerebral hemispheres.

In the light of these facts even the strictest judgement cannot raise any objection to such a physiological conclusion; at the same time, however, there is a manifest difference between the two reflexes. In the first place, their centres, as already mentioned, are different. In the second place, as is clear from the procedure of our experiments, the first reflex was reproduced without any preparation or special condition, while the second was obtained by means of a special method. This means that in the first case there took place a direct passage of the nervous current from one kind of drives to the other, without any special procedure. In the second case the passage demanded a certain preliminary procedure. The next natural assumption is that in the first reflex there was a direct conduction of the nervous current, while in the second it was necessary preliminarily to prepare the way for it; this concept had long been known

to physiology and had been termed "Bahnung."[62] Thus, in the central nervous system there are two different central mechanisms—one directly conducting the nervous current and the second—closing and opening it. There is nothing surprising in this conclusion. The nervous system is the most complex and delicate instrument on our planet, by means of which relations, connections are established between the numerous parts of the organism, as well as between the organism, as a highly complex system, and the innumerable, external influences. If the closing and opening of electric current is now regarded as an ordinary technical device, why should there be any objection to the idea that the same principle acts in this wonderful instrument? On this basis the *constant connection between the external agent and the response of the organism, which it evokes, can be rightly called an unconditioned reflex, and the temporary connection—a conditioned reflex.* The animal organism, as a system, exists in surrounding nature thanks only to the continuous equilibration of this system with the environment, i.e., thanks to definite reactions of the living system to stimulations reaching it from without, which in higher animals is effected mainly by means of the nervous system in the shape of reflexes. This equilibration, and consequently, the integrity both of the individual organism and of its species, is ensured first of all by the simplest unconditioned reflexes (such as coughing when foreign substances enter the larynx), as well as by the most complex ones, which are usually known as instincts—alimentary, defensive, sexual and others. The reflexes are caused both by internal agents arising within the organism and by external agents, and this ensures the perfection of the equilibration. But the equilibrium attained by these reflexes is complete only when there is an absolute constancy of the external environment. But since the latter, being highly varied, is always fluctuating, the unconditioned, or constant connections are not sufficient; they must be supplemented by conditioned reflexes, or temporary connections. For exam-

ple, it is not sufficient for the animal to take the food placed before it—in this case it would often be hungry and die of starvation; the animal must discover the food by its various accidental and temporary symptoms, and the latter are precisely conditioned (signalling) stimuli exciting the animal's movement towards the food which ends in its introduction into the mouth, i.e., in general, they evoke a conditioned alimentary reflex. The same holds for everything of importance for the well-being of the organism and the species both in the positive and in the negative senses, i.e., for everything which the animal must take from the environment and against which it must be on guard. No great power of imagination is needed to realize at once what a truly innumerable quantity of conditioned reflexes are constantly effected by the most complex system of the human being who is placed not only in a very broad natural environment, but often also in a very broad specifically social environment, which, on the overall scale, embraces all mankind. Let us take this alimentary reflex. How many diverse conditioned temporary connections, both generally natural and specifically social, are required by a human being to secure adequate and wholesome food—and all this is, in essence, a conditioned reflex! There is no need to explain this in greater detail. Let us make a leap and turn directly to the question of the so-called tact in life as a specifically social phenomenon. Tact means the ability to create for oneself a favourable standing in society—the quality infrequently met with, of being able to establish with everyone and in any circumstances relations that constantly evoke a generally favourable attitude; it means changing one's attitude towards other people according to their temper, sentiments and the given conditions, i.e., to react to other people depending on the positive or negative results of the previous intercourse with them. True, there is worthy and unworthy tact, the tact which does not violate self-respect and the dignity of other people, and there is the tact which is quite the reverse; but in their physiol-

ogical essence both are temporary connections, conditioned reflexes. Thus, the temporary nervous connection is the most universal physiological phenomenon both in the animal world and in ourselves. At the same time it is a psychological phenomenon—that which the psychologists call association, whether it be combinations derived from all manner of actions or impressions, or combinations derived from letters, words and thoughts. Are there any grounds for differentiation, for distinguishing between that which the physiologist calls the temporary connection and that which the psychologist terms association? They are fully identical; they merge and absorb each other. Psychologists themselves seem to recognize this, since they (at least, some of them) have stated that the experiments with conditioned reflexes provide a solid foundation for associative psychology, i.e., psychology which regards association as the base of psychical activity. This is all the more true since it is possible to form a new conditioned stimulus with the help of an elaborated conditioned stimulus; and recently it was convincingly proved on a dog that two indifferent stimuli repeated in succession can also become interconnected and provoke each other. The conditioned reflex has become the central phenomenon in physiology; it has made possible a more profound and exact study both of the normal and pathological activity of the cerebral hemispheres. Of course, the results of this study, which so far has yielded an enormous quantity of facts, can be described here only in general outline.

The basic condition for the formation of a conditioned reflex is, generally speaking, a single or repeated coincidence of the indifferent stimulus with the unconditioned one. The formation of the reflex is quickest and meets with least difficulties when the first stimulus directly precedes the second, as shown in the above-mentioned auditory acid reflex.

The conditioned reflex is formed on the basis of all unconditioned reflexes and from various agents of the inter-

nal medium and external environment both in their simplest and most complex forms, but with one limitation: it is formed only from those agents for the reception of which there are receptor elements in the cerebral hemispheres. Thus we have before us a very extensive synthesizing activity effected by this part of the brain.

But this is not enough. The conditioned temporary connection is at the same time highly specialized, reaching the heights of complexity and extending to the most minute fragmentation of the conditioned stimuli as well as of some activities of the organism, particularly such as the skeletal movements and the speech movements. Thus we have before us a highly delicate analysing activity of the same cerebral hemispheres! Hence the enormous breadth and depth of the organism's adaptability, of its equilibration with the surrounding world. The synthesis is, apparently, a phenomenon of nervous coupling. What, then, is the analysis as a nervous phenomenon? Here we have several separate physiological factors. The foundation for the analysis is provided first of all by the peripheral endings of all the afferent nervous conductors of the organism, each one of which is specially adjusted to transform a definite kind of energy (both inside and outside the organism) in the process of nervous excitation; this process is then conducted to special, less numerous, cells of the lower parts of the central nervous system, as well as to the highly numerous special cells of the cerebral hemispheres. From there, however, the process of nervous excitation usually irradiates to various cells over a greater or lesser area. This explains why when the conditioned reflex has been elaborated, say, to one definite tone, not only all the other tones, but even many of the other sounds produce the same conditioned reaction. In the physiology of the higher nervous activity this is known as the generalization of conditioned reflexes. Consequently, here we simultaneously meet with phenomena of coupling and irradiation. But afterwards the irradiation gradually becomes more and more limited; the ex-

citatory process concentrates in the smallest nervous point of the cerebral hemispheres, probably the group of corresponding special cells. This limitation is most rapidly effected by means of another basic nervous process known as inhibition. This is how the process develops. First we elaborate a conditioned generalized reflex to a definite tone. Then we continue our experiment with this reflex, constantly accompanying and reinforcing it with the unconditioned reflex; but along with it we apply other, so to speak, spontaneously acting tones, but without any reinforcement. The latter gradually lose their effect, and, finally, the same thing takes place with the closest tone; for example, a tone of 500 oscillations per second will produce an effect, whereas the tone of 498 oscillations will not, i.e., it will be differentiated. These tones, which have now lost their effect, are inhibited. This is proved in the following way:

If immediately after the application of the inhibited tone we apply the constantly reinforced conditioned tone, the latter will either produce no effect at all or a considerably lesser effect than usual. This signifies that the inhibition which has eliminated the effect of all accessory tones, has acted on this tone as well. But this is a fleeting phenomenon—it is no longer observed if some time passes after the application of the inhibited tones. From this it can be deduced that the inhibitory process irradiates in the same way as the excitatory process. But the more frequently the non-reinforced tones are repeated, the more concentrated becomes the inhibitory process both in space and in time. Consequently the analysis begins with the special activity of the peripheral mechanisms of the afferent conductors and is terminated in the cerebral hemispheres by means of the inhibitory process. The case of inhibition described above is known as differential inhibition. I shall mention other cases. In order to obtain a definite, more or less constant strength of the conditioned effect, usually, after a certain period of action of the conditioned stimulus, the latter is supplemented by an unconditioned stimulus, that is, it is

reinforced. Then, depending on the duration of the isolated application of the conditioned stimulus, no effect is observed during the first seconds or minutes of the stimulation, since being premature as a signal of the unconditioned stimulus, it is inhibited. This is the analysis of the different moments of the acting stimulus. Inhibition of this kind is called the inhibition of a delayed reflex. But the conditioned stimulus, as a signalling one, is itself corrected by the inhibition, gradually being reduced to zero, if it is not reinforced during a certain period of time.

This is the extinguishing inhibition. It persists for some time and then disappears of itself. The restoration of the extinguished conditioned effect of the stimulus is accelerated by reinforcement. Thus, there are positive conditioned stimuli, i.e., provoking an excitatory process in the cerebral cortex, and negative ones, provoking an inhibitory process. In the above cases we have a special inhibition of the cerebral hemispheres, the cortical inhibition. It arises under certain conditions at points where previously it was absent, it varies in size and disappears under other conditions; this distinguishes it from a more or less constant and stable inhibition of the lower parts of the central nervous system, and this is why, in contrast to the latter (i.e., to external inhibition), it is called internal inhibition. It would be more correct to call it elaborated, conditioned inhibition. The participation of inhibition in the work of the cerebral hemispheres is as continuous, complex and delicate as that of the excitatory process.

Just as in some cases the stimulations coming into the hemispheres from without enter into connection with definite cerebral points which are in a state of excitation, in other cases similar stimulations can, also on the basis of simultaneity, enter into temporary connection with the inhibitory state of the cortex, if there is any. This follows from the fact that such stimuli have an inhibitory effect, evoke by themselves an inhibitory process in the cortex and are conditioned negative stimuli. In this case, as in the foregoing

cases, we have a conversion, under certain conditions, of the excitatory process into the inhibitory. And this can to a degree be explained if we recall that in the peripheral apparatus of the afferent conductors there takes place a constant transformation of various kinds of energy into an excitatory process. Why, then, should there not take place in certain conditions a similar transformation of the energy of the excitatory process into the energy of the inhibitory process, and vice versa?

As we have just seen, both the excitatory and inhibitory processes, arising in the cerebral hemispheres, first spread over them or irradiate, and then concentrate in the point of origin. This is one of the fundamental laws of the entire central nervous system, but here, in the cerebral hemispheres, it manifests itself with the mobility and complexity which are inherent only in them. Among the conditions which determine the onset and course of irradiation and concentration of the processes, the strength of these processes must be considered of prime importance. The facts which have been accumulated up to date entitle us to draw the conclusion that given a weak excitatory process irradiation takes place, given a medium one—concentration, and under a very strong one—again irradiation. Exactly the same thing occurs in the inhibitory process. Cases of irradiation accompanying very strong processes are observed more seldom, and, therefore, have been less investigated, especially under inhibition. The irradiation of a weak excitatory process, being of a temporary character, discloses the latent state of excitation which is caused by another acting stimulus (but too weak to be revealed) or by a stimulus that had acted not long before, and finally by one which was often repeated and resulted in an increased tonus of a certain cortical point. On the other hand, the irradiation eliminates the inhibitory state of other points of the cortex. This phenomenon is known as disinhibition: the irradiation of an accessory weak stimulus transforms the effect of a certain acting negative conditioned stimulus into

the opposite, positive effect. When the excitatory process is of medium strength, it concentrates in a definite and limited point and is manifested in certain activity. Under very strong excitation the irradiation evokes the highest tonus of the cortex, and against the background of this excitation all other successive stimulations produce the maximum effect. The irradiation of a weak inhibitory process is what we call hypnosis; under alimentary conditioned reflexes it manifests itself in both the secretory and motor components. When, in the above-mentioned conditions there arises inhibition (differential and others), the development of peculiar states of the cerebral hemispheres is the most common fact. At first, contrary to the rule of a more or less parallel change in the size of the salivary effect of the conditioned alimentary reflexes, corresponding to the physical intensity of the stimuli, all stimuli become equal in effect (the equalization phase). Then the weak stimuli provoke a more abundant secretion of saliva than the strong (the paradoxical phase). And finally there takes place a distortion of the effects: the conditioned positive stimulus remains fully ineffective, whereas the negative stimulus produces a secretion of saliva (the ultra-paradoxical phase). The same thing occurs with the motor reaction: when, for example, food is offered to the dog (i.e., when natural conditioned stimuli begin to act), the dog turns away from it; on the contrary, when the food is being removed, taken away, the dog reaches for it. Besides, in the state of hypnosis, in the case of alimentary conditioned reflexes, it is sometimes possible clearly to observe a gradual irradiation of inhibition over the motor region of the cortex. First the tongue and the masticatory muscles become paralyzed, then the inhibition of the cervical muscles follows, and, finally, of all muscles of the body. Given a further downward irradiation of the inhibition along the brain a state of catalepsy is sometimes observed, and finally general sleep sets in. The hypnotic state, being of an inhibitory nature, enters quite easily, on the basis of simultaneity, into tem-

porary conditioned connection with the numerous external agents.

When the inhibitory process is intensified, it becomes concentrated. This leads to delimitation between the cortical point which is in a state of excitation and the points in a state of inhibition. And since there is a multitude of diverse points in the cortex, excitatory and inhibitory, relating both to the external world (visual, auditory and others) and to the internal world (motor, etc.), it represents a grandiose mosaic of intermittent points of various properties and various degrees of strength of the excitatory and inhibitory states. Thus, the alert working state of an animal or of a human being is a mobile and at the same time localized process of fragmentation of the excitatory and inhibitory states of the cortex, now in large, now in very small parts; it contrasts with the state of sleep when inhibition at the height of its intensity and extensity is spread evenly over the whole mass of the cerebral hemispheres, as well as down to a certain level. However, even then there may remain separate excitatory points in the cortex—which are, so to speak, on guard or on duty. Consequently, in the alert state both processes are in permanent mobile equilibration, as if struggling with each other. If the mass of external or internal stimulations falls off at once, a marked predominance of the inhibitory process takes place over the excitatory. Some dogs, in which the peripheral basic external receptors (visual, auditory and olfactory) are damaged, sleep twenty-three hours a day.

Along with the law of irradiation and concentration of the nervous processes, there is another permanently operating fundamental law—the law of reciprocal induction. According to this law, the effect of the positive conditioned stimulus becomes stronger when the latter is applied immediately or shortly after the concentrated inhibitory stimulus, just as the effect of the inhibitory stimulus proves to be more exact and profound after the concentrated positive stimulus. The reciprocal induction manifests itself both

in the circumference of the point of excitation or inhibition simultaneously with their action, and in the point itself after the termination of the processes. It is clear that the law of irradiation and concentration and the law of reciprocal induction are closely interconnected, mutually limiting, balancing and reinforcing each other, and thereby determining the exact correlation between the activity of the organism and the conditions of the external environment. Both laws operate in all parts of the central nervous system, but in the cerebral hemispheres they manifest themselves in newly arising points of excitation and inhibition, and in the lower parts of the central nervous system—in more or less permanent points. In the theory of conditioned reflexes, negative induction, i.e., the emergence or intensification of inhibition in the circumference of a point of excitation, was previously called external inhibition, when the given conditioned reflex diminished and disappeared as a result of the action on the animal of an accessory, accidental stimulus, more often evoking an orienting reflex. It was this that gave the occasion to group all the cases of inhibition described above (extinguishing and others), occurring without the interference of outside stimulation, under the common name of internal inhibition. Besides these two different cases of inhibition in the cerebral hemispheres, there is a third one. When the conditioned stimuli are physically very strong, the rule of direct proportionality between the strength of the effect produced by these stimuli and their physical intensity is violated; their effect becomes not stronger but weaker than that of moderate stimuli; this is the so-called transmarginal inhibition. This inhibition arises both under the action of a very strong conditioned stimulus and in the case of summation of separate and not very strong stimuli. It is natural to regard transmarginal inhibition as a kind of reflex inhibition. If we systematize the cases of inhibition more exactly, we shall have either permanent, unconditioned inhibition (inhibition of negative induction and transmarginal inhibition), or temporary, conditioned inhi-

bition (extinguishing, differential and retarding). However, from the point of view of their physicochemical foundation, there is every reason to regard all these kinds of inhibition as one and the same process, but arising under different conditions.

The entire establishment and distribution in the cortex of excitatory and inhibitory states, taking place in a certain period under the action of external and internal stimuli, become more and more fixed under uniform, recurring conditions and are effected with ever-increasing ease and automatism. Thus, there appears a dynamic stereotype (systematization) in the cortex, the maintenance of which becomes an increasingly less difficult nervous task; but the stereotype becomes inert, little susceptible to change and resistant to new conditions and new stimulations. Any initial elaboration of a stereotype is, depending on the complexity of the system of stimuli, a difficult and often an extraordinary task.

The study of conditioned reflexes in numerous dogs gradually led to the idea of different nervous systems in different animals, until, finally, sufficient data were obtained to systematize the nervous systems according to some of their basic properties. There proved to be three such properties: the strength of the basic nervous processes (excitatory and inhibitory), their equilibrium and their mobility. Actual combinations of these three properties produce four more or less strongly-pronounced types of nervous system. According to the strength, the animals are divided into strong and weak types; according to the equilibrium of the nervous processes, the strong animals are divided into equilibrated and unequilibrated; and the equilibrated strong animals are divided into labile and inert. This, approximately, coincides with the classical systematization of temperaments. Thus, there are strong but unequilibrated animals in which both nervous processes are strong, the excitatory process, however, predominating over the inhibitory; this is the excitable, impetuous type, or choleric, ac-

cording to Hippocrates. Further, there are strong, quite equilibrated but inert animals; this is the inert, slothful type, or phlegmatic, according to Hippocrates' classification. Then come the strong, quite equilibrated, but labile animals; this is the lively, active type, or sanguine, according to Hippocrates. And finally, there is the weak type, which is closest to Hippocrates' melancholic type; the predominant and common feature of this type is quick inhibitability due to internal inhibition which is always weak and easily irradiates, and especially to external inhibition under the action of various, even inconsiderable, accessory external stimuli. In other respects it is less uniform than all other types; it includes various animals: those in which both nervous processes are equally weak; those in which the inhibitory process is predominantly very weak; fussy animals, constantly glancing around, and, on the contrary, animals constantly halting, as if becoming petrified. The cause of this non-uniformity lies, of course, in the fact that animals of the weak type, as well as those of the strong type, differ in other features, apart from the strength of the nervous processes. But the predominant and extreme weakness now of the inhibitory process, now of both processes, abolishes the vital significance of the variations of all other features. Constant and strong inhibitability makes all these animals equally disabled.

Thus, type is a congenital, constitutional form of the nervous activity of the animal—the genotype. But since the animal is exposed from the very day of its birth to the most varied influences of the environment, to which it must inevitably respond by definite actions which often become more and more fixed and, finally, established for life, the ultimate nervous activity of the animal (phenotype, character) is an alloy of the characteristics of type and the changes produced by the external environment. All that has been said above, obviously, represents indubitable physiological material, i.e., the objectively reproduced normal physiological activity of the higher part of the central nervous system;

and it is precisely with this activity that the study of every part of the animal organism must begin and actually does begin. However, this does not prevent certain physiologists from regarding the above facts as having no relation to physiology. A case of conservatism not infrequent in science!

It is not difficult to bring this physiological activity of the higher part of the animal brain into natural and direct connection with numerous manifestations of our subjective world.

As already mentioned, a conditioned connection is, apparently, what we call association by simultaneity. The generalization of a conditioned connection corresponds to what is called association by likeness. The synthesis and analysis of conditioned reflexes (associations) are, in essence, the same as the basic processes of our mental activity. When we are absorbed in our thoughts or carried away by certain work, we do not see and hear what is going on around us; this is obvious negative induction. Who would separate in the unconditioned highly complex reflexes (instincts) the physiological, the somatic from the psychical, i.e., from the powerful emotions of hunger, sexual attraction, anger, etc.? Our sense of pleasure, displeasure, composure, difficulty, joy, pain, triumph, despair, etc., is connected now with the conversion of very strong instincts and of their stimuli into corresponding effector acts, now with their inhibition; they are connected with all the variations of an easy or difficult course of development of the nervous processes in the cerebral hemispheres, as is observed in dogs which are able or unable to cope with nervous tasks of varying degrees of difficulty. Our contrasting emotions are, of course, phenomena of reciprocal induction. The irradiation of excitation makes us speak and act in a manner that would not be admitted by us in a state of calm. Obviously, the wave of excitation transforms the inhibition of certain points into a positive process. A drastic weakening of the memory for the near past—a normal phenome-

non in old age—signifies a senile decrease of the mobility of the excitatory process, its inertness, and so on.

When the developing animal world reached the stage of man, an extremely important addition was made to the mechanisms of the nervous activity. In the animal, reality is signalized almost exclusively by stimulations and by the traces they leave in the cerebral hemispheres, which come directly to the special cells of the visual, auditory or other receptors of the organism. This is what we, too, possess as impressions, sensations and notions of the world around us, both the natural and the social—with the exception of the words heard or seen. This is the first system of signals of reality common to man and animals. But speech constitutes a second signalling system of reality which is peculiarly ours, being the signal of the first signals. On the one hand, numerous speech stimulations have removed us from reality, and we must always remember this in order not to distort our attitude to reality. On the other hand, it is precisely speech which has made us human, a subject on which I need not dwell in detail here. However, it cannot be doubted that the fundamental laws governing the activity of the first signalling system must also govern that of the second, because it, too, is activity of the same nervous tissue.

The most convincing proof that the study of the conditioned reflexes has brought the investigation of the higher part of the brain on to the right trail and that the functions of this part of the brain and the phenomena of our subjective world have finally become united and identical, is provided by the further experiments with conditioned reflexes on animals reproducing pathological states of the human nervous system—neuroses and certain psychotic symptoms; in many cases it is also possible to attain a rational deliberate return to the normal—recovery—i.e., a truly scientific mastery of the subject. Normal nervous activity is a balance of all the above-described processes participating in this activity. Derangement of the balance is a patholog-

ical state, a disease; and often there is a certain disequilibrium even in the so-called normal, or to be more precise, in the relative normal. Hence the probability of nervous illness is manifestly connected with the type of nervous system. Under the influence of difficult experimental conditions those of our dogs are quickly and easily susceptible to nervous disorders which belong to the extreme—excitable and weak—types. Of course, even in the strong equilibrated types the equilibrium can be deranged by applying very strong, extraordinary measures. The difficult conditions, which chronically violate the nervous equilibrium, include: overstrain of the excitatory process, overstrain of the inhibitory process and a direct collision of both opposite processes, in other words, overstrain of the mobility of these processes. We have a dog with a system of conditioned reflexes to stimuli of different physical intensity, positive and negative reflexes which are called forth stereotypically in one and the same order and at the same intervals. We sometimes apply exceptionally strong conditioned stimuli, sometimes we greatly prolong the duration of the inhibitory stimuli; we now elaborate a very delicate differentiation, now increase the quantity of inhibitory stimuli in the system of reflexes; finally, we either make the opposing processes follow each other immediately, or even simultaneously apply opposite conditioned stimuli, or at once change the dynamic stereotype, i.e., convert the established system of conditioned stimuli into an opposite series of stimuli. And we see that in all these cases the above-mentioned extreme types fall with particular ease into chronic pathological states differently manifesting themselves in these types. In the excitable type the neurosis is expressed in the following way. The inhibitory process, which even in a normal state constantly lags behind the excitatory process in relation to strength, now becomes very weak, almost disappearing: the elaborated, although not absolute, differentiations become fully disinhibited; the extinction assumes an extremely protracted character, the delayed reflex is

converted into a short-delayed one, etc. In general, the animal becomes highly unrestrained and nervous during the experiments in the stand: it either behaves violently, or—which is much less frequent—falls into a state of sleep; this had not been observed before. In the weak type the neurosis is almost exclusively of a depressive character. The conditioned reflex activity becomes highly confused, and more often completely vanishes; in the course of the experiment the animal is in an almost continuous hypnotic state, manifesting its various phases (there are no conditioned reflexes at all, the animal even refuses food).

Experimental neuroses in most cases assume a lingering character lasting for months and even years. Some therapeutic remedies have been successfully tested in protracted neuroses. Already long ago bromide was applied in the study of the conditioned reflexes when certain experimental animals could not cope with the tasks of inhibition. And it was of essential help to these animals. A prolonged and diverse series of experiments with conditioned reflexes on animals proved beyond all doubt that bromide bears no special relation to the excitatory process and does not decrease the latter, as was generally believed, but influences the inhibitory process, intensifying and tonifying it. It is a powerful remedy, regulating and rehabilitating the disturbed nervous activity, on the indispensable and essential condition, however, that it is exactly dosed according to the types and states of the nervous system. In the case of a strong type and when the state of the dog's nervous system is still strong enough, large doses of bromide are to be administered—from two to five grammes a day; for the weak type the dose must be reduced to centigrammes and milligrammes. Such bromization for a period of two or three weeks sometimes proves sufficient to cure a chronic experimental neurosis. Recent experiments have shown even a greater therapeutic effect, especially in very severe cases, of a combination of bromide and caffeine, but again subject to very precise dosage of both substances. Sometimes recov-

ery was also attained in animals, though not so quickly and fully, exclusively by means of a regular prolonged or short rest from laboratory work in general, or by the abolition of the difficult tasks in the system of conditioned reflexes.

The described neuroses in animals can best be compared with neurasthenia in human beings, especially since some neuropathologists insist on two forms of neurasthenia—excitatory and depressive. Besides, certain traumatic neuroses may correspond to them, as well as other reactive pathological states. It may be assumed that recognition of two signalling systems of reality in man will lead specially to an understanding of the mechanisms of two human neuroses—hysteria and psychasthenia. If, on the basis of the predominance of one system over the other, people can be divided into a predominantly thinking type and a predominantly artistic type, then it is clear that in pathological cases of a general disequilibrium of the nervous system, the former will become psychasthenics and the latter hysterics.

Along with elucidation of the mechanisms of neuroses, the physiological study of the higher nervous activity provides a clue to an understanding of certain aspects and phenomena in the pictures of psychoses. We shall dwell first of all on some forms of delusion, namely, on the variation of the persecution delusion, on what Pierre Janet[63] calls "senses of possession," as well as on Kretschmer's[64] "inversion." The patient is persecuted precisely by that which he particularly wants to avoid; he desires to have his own secret thoughts, but he is certain that they are constantly being disclosed and made known by others; he wishes to be alone, but he is tormented by the persistent sensation that someone else is in the room, although there is nobody there except himself, etc.; according to Janet, these are senses of possession. Kretschmer refers to two girls who, having entered the period of puberty, and being sexually attracted by certain males, for some reason suppressed this attraction. As a result, they were first seized with

an obsessive idea; to their great grief, it seemed to them that their countenance betrayed their sexual excitation and that everybody noticed this; at the same time they greatly valued their chastity, their virginity. Afterwards one of the girls suddenly began to imagine and even to sense that the sexual tempter—the serpent which had seduced Eve in the Garden of Eden—was inside her and was even reaching towards her mouth. The other girl imagined that she was pregnant. It is this latter phenomenon that Kretschmer terms inversion. In respect of its mechanism it is obviously identical with the sense of possession. This pathological subjective experience can, without undue strain, be interpreted as a physiological phenomenon of the ultra-paradoxical phase. The idea of sexual inviolability, being a very strong positive stimulus, on the background of the state of inhibition or depression in which both girls found themselves, turned into an equally strong opposite negative idea, reaching the level of sensation; in one girl it was the idea of a sexual tempter existing inside her body, in the other—the idea of pregnancy as a result of sexual intercourse. Exactly the same thing is experienced by the patient with the sense of possession. The strong positive idea "I am alone" turned, under the same conditions, into a similar negative idea—"there is always someone near me!"

In the course of experiments with conditioned reflexes in various difficult and pathological states of the nervous system it is often observed that temporary inhibition leads to a temporary improvement in these states; in one dog there was twice observed a patent catatonic state,[65] which resulted in a marked decline of a chronic and persistent nervous disorder, almost in a return to the normal for several days in succession. In general, it should be pointed out that in experimental disorders of the nervous system almost always separate phenomena of hypnosis are observed, which gives the right to assume that this is a normal physiological remedy against morbific agents. Hence, the catatonic form or phase of schizophrenia[66] entirely consist-

ing of hypnotic symptoms, can be regarded as physiological protective inhibition, limiting or fully excluding the work of the disordered brain which, owing to the action of a certain, still unknown, noxious agent, has been threatened by serious disturbances or complete destruction. Medicine knows very well that the first therapeutic measure, which must be applied in the treatment of almost every illness is to ensure a state of rest for the diseased organ. That such a concept of the mechanism of catatonia in schizophrenia conforms to reality, is convincingly proved by the fact that only this form of schizophrenia shows a considerable rate of recovery, despite the protracted character of the catatonic state, which sometimes persists for years (twenty years). From this point of view any attempt to act on catatonics by means of stimulating methods and remedies is definitely injurious. On the contrary, a very considerable increase in the rate of recovery can be expected when physiological rest (inhibition) is supplemented with deliberate external rest for such patients, when they are kept away from the action of constant and strong stimuli emanating from the surroundings, kept away from other, restless patients.

In the course of the study of conditioned reflexes, along with general disorders of the cortex, there were frequently observed extremely interesting cases of disorders experimentally and functionally produced in very small points of the cortex. Let us take a dog with a system of various reflexes and among them conditioned reflexes to different sounds—a tone, a noise, the beat of a metronome, the sound of a bell, etc.; it is possible to induce a disorder only at one of the points of application of these conditioned stimuli, while all other points remain normal. The pathological state of an isolated cortical point is produced by the methods described above as morbific. The disorder manifests itself in different forms and degrees. The mildest change effected at this point is expressed in its chronic hypnotic state: instead of the normal relation between the strength of the effect induced by the stimulation and the phys-

ical intensity of the stimulus, the equalization and paradoxical phases develop at this point. Proceeding from the above, this, too, can be interpreted as a physiological preventive measure under a difficult state of a cortical point. When the pathological state develops further, the stimulus in some cases has no positive effect at all, provoking only inhibition. In other cases the opposite occurs. The positive reflex becomes unusually stable: its extinction proceeds more slowly than that of the normal reflexes; it is less susceptible to successive inhibition by other, inhibitory conditioned stimuli; it often stands out in bold relief for its strength among all other conditioned reflexes, which was not observed prior to the disorder. This signifies that the excitatory process at the given point has become chronically and pathologically inert. The stimulation of the pathological point sometimes remains indifferent to the points of other stimuli, and sometimes it is impossible to touch this point with its stimulus without deranging in one way or another the entire system of reflexes. There are grounds for assuming that in the case of disorder of isolated points, when now the inhibitory, now the excitatory processes predominate at the diseased point, the mechanism of the pathological state consists precisely in the derangement of equilibrium between the opposed processes: there takes place a considerable and predominant decrease now of one process, now of the other. In the case of pathological inertness of the excitatory process bromide (which reinforces the inhibitory process) often fully eliminates the inertness.

The following conclusion can hardly be considered fantastic. If stereotypy, iteration and perseveration, as is perfectly obvious, have their natural origin in the pathological inertness of the excitatory process of the different motor cells, then obsessional neurosis and paranoia must also have the same mechanism. This is simply a matter of other cells or of groups of cells connected with our sensations and notions. Thus, only one series of sensations and notions connected with the diseased cells becomes abnormally stable

and resistant to the inhibitory influence of other numerous sensations and notions, which to a greater degree conform with reality because of the normal state of their cells. Another phenomenon, frequently observed in the study of pathological conditioned reflexes and having a direct bearing on human neuroses and psychoses, is circularity in the nervous activity.[67] The disturbed nervous activity manifested more or less regular fluctuations. There was observed at first a period of extremely weakened activity (the conditioned reflexes were of a chaotic character, often fully disappeared or declined to the minimum); then, after several weeks or months, as if spontaneously, without any visible reason, there took place a greater or lesser, and even complete, return to the normal, which was again superseded by a period of pathological activity. Sometimes periods of weakened activity and abnormally increased activity alternated in this circularity. It is impossible not to see in these fluctuations an analogy with cyclothymia[68] and the manic-depressive psychosis.[69] The simplest way would be to ascribe this pathological periodicity to the derangement of normal relations between the excitatory and inhibitory processes, as far as their interaction is concerned. Since the opposite processes did not limit each other in due time and in the proper measure, but acted independently of each other and excessively, the result of their activity reached its maximum—and only then was one process superseded by the other. Thus, there developed a different, namely, exaggerated, periodicity, lasting a week or a month, instead of the short and very easy periodicity of one day. Finally, it is impossible not to mention a phenomenon which so far has manifested itself with exceptional force only in one dog. This is the extreme explosiveness of the excitatory process. Certain individual stimuli or all the conditioned stimuli produced an extremely violent and excessive effect (both motor and secretory), which, however, abruptly disappeared already during the action of the stimulus—when the alimentary reflex was reinforced, the dog did not take

the food. Obviously, this was because of the high pathological lability of the excitatory process, which corresponds to the excitatory weakness of the human clinic. In certain conditions a weak form of this phenomenon is often observed in dogs.

All the pathological nervous symptoms described above are manifested in corresponding conditions both in normal dogs, i.e., not subjected to surgical operation, and (especially some of these symptoms, for example, circularity) in castrated animals, being, consequently, of an organic pathological nature. Numerous experiments have shown that the most fundamental property of the nervous activity in castrated animals is a considerable and predominant decline of the inhibitory process, which in the strong type, however, is greatly levelled out with the passage of time.

To sum up, we must emphasize once more that when we compare the ultra-paradoxical phase with the sense of possession and with inversion, and the pathological inertness of the excitatory process with obsessional neurosis and paranoia, we see how closely the physiological phenomena and the experiences of the subjective world are interconnected and how they merge.

PHYSIOLOGY OF THE HIGHER NERVOUS ACTIVITY[70]

Since this, I suppose, is my last opportunity to address a general meeting of my colleagues, I shall take the liberty of calling your attention to the general, most systematized and summarized results of my recent work, which I have carried out jointly with my esteemed fellow-workers and which comprises a full half of my entire physiological activity; naturally, I shall repeat many of the already published facts. I pass on to you the results of our work, passionately dreaming of the majestic, ever-widening horizon opening up before our science, and of the ever-growing influence exerted by science on human nature and human destiny.

For the anatomist and histologist the cerebral hemispheres have always been as accessible and tangible as any other organ or any other tissue, i.e., that they possess similar workability and are susceptible of investigation, but, of course, commensurately with their specific properties and construction. Quite different was the position of the physiologist. Every organ of the animal body, the general role of which in the organism is known, its actual function, and the conditions and mechanism of this function, are objects of study. As to the cerebral hemispheres, their role is well known—they effect the organism's most complex relations with the environment; but the physiologist did not engage in a further study of their activity. For him the study of the cerebral hemispheres did not begin with the concrete reproduction of their activity, only after which the gradual ana-

lysis of the conditions and mechanism of this activity is possible. The physiologist possessed many facts relating to the cerebral hemispheres, but these facts were not manifestly and closely connected with their usual normal activity.

Today, after thirty years of diligent and ceaseless work jointly with my numerous collaborators, I make bold to say that the situation has radically changed, that while remaining physiologists, i.e., the same objective observers as in all other branches of physiology, we are studying at present the normal activity of the cerebral hemispheres and at the same time constantly analysing it in ever-increasing measure. The generally-recognized criteria for every true scientific activity, namely, precise prevision and control over phenomena, testify to the serious character of this study, which is irrepressibly advancing, overcoming all obstacles. An ever-growing number of relations which constitute the most complex external activity of the higher animal organism, unfolds before us.

The central physiological phenomenon in the normal work of the cerebral hemispheres is that which we have termed the *conditioned reflex*. This is a temporary nervous connection between numberless agents in the animal's external environment, which are received by the receptors of the given animal, and the definite activities of the organism. This phenomenon is called by psychologists *association*. The fundamental physiological significance of this connection is as follows: in the higher animal, for example, in the dog which was the object of our investigations, the basic, most complex correlations established between the organism and the environment in order to preserve the individual and the species, are determined first of all by the activities of the subcortex which is nearest to the cerebral hemispheres; this was demonstrated long ago in Goltz's[71] experiment with the extirpation of the cerebral hemispheres in a dog. These activities include the search for food, or the alimentary activity; the avoidance of injurious factors, or the defensive activity, etc. They are usually called instincts

or inclinations; psychologists term them *emotions*, but we designate them by the physiological term *most complex unconditioned reflexes*. They exist from the very day of birth and are indispensably called forth by definite, though very limited in number, stimuli which are sufficient only in early childhood, under the conditions of parental care. It is this latter circumstance that makes an animal with extirpated cerebral hemispheres disabled, incapable of a self-dependent existence. The basic physiological function of the cerebral hemispheres throughout the subsequent individual life consists in a constant addition of numberless signalling conditioned stimuli to the limited number of the initial, inborn unconditioned stimuli, in other words, in constantly supplementing the unconditioned reflexes by conditioned ones. Thus, the objects of the instincts exert an influence on the organism in ever-widening regions of nature and by means of more and more diverse signs or signals, both simple and more complex; consequently, the instincts are more and more fully and perfectly satisfied, i.e., the organism is more reliably preserved in the surrounding nature.

The basic condition for the formation of a conditioned reflex is a single or repeated coincidence in time of indifferent stimuli with unconditioned reflexes. This is the same principle of coincidence in time, on the basis of which groups of various agents or elements of nature, both simultaneous and consecutive, are synthesized by the animal into units. In this way the *synthesis* is effected in general.

But owing to the complexity of the permanent movement and variation of the natural phenomena, the conditioned reflex must, of course, also undergo certain changes, i.e., be constantly corrected. If for some reason or other the conditioned stimulus in the given conditions is not accompanied by its unconditioned stimulus, then, when repeated, it quickly loses its effect, however, temporarily, being restored spontaneously, after a certain lapse of time. If the conditioned stimulus constantly and greatly precedes in time the moment when the unconditioned stimulus is added,

then its distant part, which is, so to speak, premature and violates the principle of economy, proves ineffective. When the conditioned stimulus, connected with another indifferent one, is permanently not accompanied by an unconditioned stimulus, it remains, in this combination, ineffective. Finally, if agents closely akin to the given elaborated conditioned stimulus (for example, close tones, other spots of the skin, etc.) are usually effective immediately after the elaboration of the first one, they gradually lose their effect when repeated later on without the accompaniment of the unconditioned stimulus, or, in our usual terminology, without reinforcement. All this ensures the differentiation, the *analysis* of the surrounding world with all of its elements and moments.

In the long run, the cerebral hemispheres of the dog constantly effect in the most varying degrees both the *analysis* and *synthesis* of stimuli coming to them, and this can and must be termed *elementary, concrete thinking*. And it follows that this thinking is responsible for the perfect adaptation of the organism, for its more delicate equilibration with the environment.

This real activity of the cerebral hemispheres and of the nearest subcortex, just described in general outline, the activity which ensures normal complex relations between the organism as a whole and the external world, must be rightly considered and denoted as the *higher nervous activity*, the external behaviour of the animal, instead of "psychical" as it was termed previously; it should be distinguished from the activity of other parts of the brain and of the spinal cord which are mainly in charge of the correlations and integration of separate parts of the organism; this activity should be termed the *lower nervous activity*.

Now the following questions arise: what intrinsic processes and laws govern the higher nervous activity? What has it in common with, and how does it differ from, the lower nervous activity which until now has been the predominant object of physiological study?

The basic processes of the entire central nervous activity are, obviously, always the same, namely, the excitatory and inhibitory processes. There are sufficient grounds for assuming that the fundamental laws governing these processes are also of a constant nature—irradiation and concentration of the processes and their reciprocal induction.

It seems to me that experiments with conditioned reflexes on the cerebral hemispheres, given normal conditions, permit a more complete and exact formulation of these laws than was possible on the basis of experiments performed mainly on the lower parts of the central nervous system, and which, in most cases, were acute experiments.

Concerning the cerebral hemispheres we can say that the following phenomenon is observed in them: when the excitatory and inhibitory processes are weak, then, under the action of corresponding stimuli there takes place irradiation, diffusion of the processes from the point of origin; when they are of medium strength, a concentration of the processes occurs at the point of application of the stimulus, and when they are very strong irradiation is again in evidence.

In the entire central nervous system, on the basis of irradiation of the excitatory process, a summation reflex sets in, i.e., a summation of the spreading wave of excitation with a local manifest or latent excitation; in the latter case the latent tonus becomes revealed—a phenomenon already known for a long time. While in the cerebral hemispheres the confluence of waves irradiating from various points leads to a quick development of a temporary connection, to an association of these points, it bears a momentary, transient character in the remaining part of the central nervous system. This connection in the cerebral hemispheres probably owes its emergence to their extremely high reactivity and ability to impress, and is a permanent and inherent property of this part of the central nervous system. Moreover, in the cerebral hemispheres the irradiation of the excitatory process instantly and for a short period of time

eliminates, washes off the inhibition from the inhibitory, negative points of the hemispheres, converting these points for the same period of time into positive ones. This phenomenon is called disinhibition.

Under the irradiation of the inhibitory process there is observed a decline or complete disappearance of the effect of the positive points and an increased effect of the negative points.

When the excitatory and inhibitory processes are concentrated, they induce the opposite processes (both at the periphery during their action and in the place of action upon its termination); this is the law of reciprocal induction.

In the entire central nervous system when there is a concentration of the excitatory process, we meet with phenomena of inhibition. The point of concentration of the excitation is encircled to a greater or lesser extent by the inhibitory process; this is the phenomenon of negative induction. This phenomenon manifests itself in all reflexes, develops at once and in full measure, persists for some time after the termination of excitation and exists both between the small points and the large parts of the brain. We call this external, passive, unconditioned inhibition. This phenomenon, which has also been known for a long time, was sometimes called the conflict of centres.

There are in the cerebral hemispheres also other kinds or cases of inhibition, in all probability, having one and the same physicochemical substratum. This is, in the first place, the inhibition effecting the correction of the conditioned reflexes, already mentioned and arising when the conditioned stimulus in the above-indicated conditions is not accompanied by its unconditioned stimulus; it gradually grows, becomes stronger and can be trained and perfected; this, too, is due to the exceptional reactivity of the cortical cells, and hence to the particular lability of inhibition in them. We call this inhibition internal, active, conditioned. The stimuli, which are thus converted into permanent agents of inhibition in the points of the cerebral

hemispheres, are called by us inhibitory, negative. Similar inhibitory stimuli can be also obtained in another way—if we repeatedly apply indifferent stimuli during the inhibitory state of the cerebral hemispheres (experiments of Prof. Volborth). As is known, the initial inhibitory reflexes are also developed in the lower parts of the brain and in the spinal cord; but here they appear at once in a finished and stereotyped form, while the same inhibitory reflexes of the cerebral hemispheres arise gradually and are always observed by us in the process of formation.

There is one more case of inhibition in the cerebral hemispheres. All other conditions being equal, the effect of conditioned stimulation, as a rule, is proportionate to the intensity of the physical strength of the stimulus, but to a certain maximum (and probably to a certain minimum, too). Beyond this limit the effect does not increase; it either remains unchanged or declines. We have grounds for assuming that beyond this margin the stimulus together with the excitatory process evoke also an inhibitory process. We interpret this fact in the following way. The cortical cell possesses a certain limit of efficiency, and beyond this point there arises inhibition which prevents an excessive functional exhaustion of the cell. The limit of efficiency is not constant; it undergoes both acute and chronic changes—in cases of inanition, hypnosis, disease and in old age. This inhibition, which can be called transmarginal, arises sometimes instantaneously and sometimes manifests itself only when the super-powerful stimuli are repeated. It can be assumed that analogical inhibition also exists in the lower parts of the central nervous system.

Peculiar internal inhibition could also be considered as transmarginal inhibition, in which case the intensity of excitation is, as it were, replaced by its long duration.

Any inhibition irradiates in the same way as excitation, but the irradiation of internal inhibition is particularly distinct in the cerebral hemispheres where it is very easily observed in various forms and degrees.

There is no doubt that inhibition, when spreading and deepening, calls forth different degrees of a hypnotic state, and when irradiating to the utmost from the cerebral hemispheres down the brain, produces normal sleep. Particularly manifest, even in our dogs, is the diversity and multiplicity of the stages of hypnosis, which at first hardly differs from the wakeful state. In respect of intensity of inhibition the following stages are worth mentioning: the so-called equalization, paradoxical and ultra-paradoxical phases. Now conditioned stimuli of different physical strength produce either an equal, or even an inversely proportional effect; in rare cases only the inhibitory stimuli act positively, and the positive stimuli are converted into inhibitory ones. In respect of extensity of inhibition, functional dissociations in the cortex itself are observed, as well as between the cortex and the lower parts of the brain. In the cortex the motor region is particularly often isolated from other regions, and even within this region a distinct functional dissociation sometimes comes to the fore.

Unfortunately, the rivalry of what the clinicians and some experimenters designate "the centre of sleep" prevents these facts from being generally recognized and properly utilized for an understanding of the multitude of physiological and pathological phenomena. However, it is not difficult to reconcile and combine these facts. Sleep can be originated in two ways—either by irradiation of inhibition from the cortex, or by limiting the stimulations reaching the higher parts of the brain both from without and from within the organism. Strümpel long ago produced sleep in a patient by means of drastic limitation of external stimulations.[72] Recently Prof. Speransky and Galkin by means of a peripheral destruction of the olfactory, auditory and visual receptors in dogs obtained a very profound and chronic sleep (lasting weeks and months). Similarly, as a result of a pathological or experimental exclusion of stimulations, constantly reaching the higher part of the brain there sets in due to the vegetative activity of the organism an exag-

gerated and more or less profound and chronic sleep. It can be recognized that in some of these cases too, sleep, in the final stage, is produced by similar inhibition which becomes predominant when the number of stimuli is limited.

The law of reciprocal induction begins to operate when there is a concentration of the inhibitory process, just as it does when there is a concentration of the excitatory process. The point of concentration of the inhibition is to a greater or lesser extent encircled by the process of heightened excitability; this is the phenomenon of positive induction. The heightened excitability arises either instantly or gradually and persists not only during the action of inhibition, but for some time after, and in some cases even for a quite considerable length of time. The positive induction manifests itself between the small points of the cortex, when the inhibition is fragmentary, as well as between the large parts of the brain, when it is more diffused.

The permanent operation of the above-mentioned laws helps us to understand the mechanism of the origin of the numerous separate phenomena (among which are many peculiar, at first sight enigmatic, phenomena) of the higher nervous activity; however, I cannot dwell on them here. I shall refer only to one of a series of similar cases which for a long time completely baffled comprehension. It relates to the complex influence of accessory stimuli on the delayed conditioned reflex (experiments performed a long time ago by our colleague Zavadsky).

Let us suppose that a delayed conditioned reflex is being elaborated, the conditioned stimulation constantly lasting three minutes before the unconditioned stimulus is added to it. When such a reflex has been elaborated, the conditioned stimulus does not produce any effect during the first minute. Half-way through or towards the end of the second minute the stimulus begins to produce a certain effect, and maximum effect is attained only during the third minute. Thus, the conditioned reflex consists of two external phases —ineffective and effective. Special experiments, however,

have established that the first phase is not a zero phase, but an inhibitory one.

Now, if simultaneously with the conditioned stimulus there are applied accessory stimuli of different intensity calling forth only an orienting reaction, a number of changes are observed in the delayed reflex. When the stimulation is weak the ineffective phase becomes effective, that is, the special effect of the conditioned stimulus is manifested; the effect of the second phase either remains unchanged or is slightly increased.

When the stimulation is more intense the same thing occurs with the first phase, but the effect of the second phase drastically declines. Under the strongest stimulation the first phase again remains ineffective, while the effect of the second completely disappears. At present, on the basis of the latest, not yet published, experiments carried out by our colleague Rickman, we interpret all these phenomena as a result of the operation of the following four laws: 1) irradiation of the excitatory process, 2) negative induction, 3) summation, and 4) the law of maximum. Given a weak orienting reflex the spreading wave of excitation eliminates the inhibition of the first phase; this reflex, which soon all but disappears when the same stimulation is continued, either does not influence the second phase at all, or, owing to a slight summation, somewhat intensifies it. With a more considerable orienting reflex the effect persists longer; consequently, along with the disinhibition of the first phase, due to a considerable summation of the effective phase of the conditioned reflex with the irradiated wave of excitation of the orienting reflex, transmarginal inhibition takes place during the last minute of the delayed reflex. Finally, given a very strong orienting reflex there takes place a complete concentration of excitation accompanied by a strong negative induction which merges with the inhibition of the first phase and abolishes the effective phase.

Despite the fact that a multitude of particular relations between the excitatory and inhibitory processes have been

studied by us, the general law of the interconnection of these processes cannot, as yet, be exactly formulated. As for the profound mechanism of both processes, many of our experimental facts incline us to the point of view that the inhibitory process is probably connected with assimilation, just as the excitatory process is naturally connected with dissimilation.

As for the so-called *voluntary volitional movements*, in this field, too, we have accumulated some material. In keeping with earlier investigations we have shown that the motor region of the cortex is first of all a receptor one, like all its other regions—visual, auditory, etc., since the animal's passive movements, i.e., the kinesthetic stimuli of this region can be transformed by us into conditioned stimuli in the same way as all external stimuli. Another ordinary phenomenon, reproduced by us also in the laboratory, is the temporary connection established between various external stimuli and passive movements which in response to certain signals evokes definite active movements of the animal. However, it is still not clear whether the connection between the kinesthetic stimulus and the corresponding motor action is of an unconditioned or of a conditioned character. Beyond this extreme point the *entire mechanism of volitional movement is a conditioned associative process* which obeys all the above-mentioned laws of the higher nervous activity.

The cerebral hemispheres are continually receiving countless stimuli both from the external world and the internal medium of the organism itself. These stimuli are conducted from the periphery along definite and numerous paths and, consequently, they first of all come to definite points and areas in the mass of the brain. Thus we have before us in the first place a highly complex structure, a mosaic. Countless and varied positive processes enter the cortex along the conductor paths, and in the cortex itself they are joined by inhibitory processes. From each of the separate states of the cortical cells (and there is an infinite number of such states) a specific conditioned stimulus may arise, as

constantly observed by us in the course of our investigation of the conditioned reflexes. All these meet, collide, must come together and be systematized. Thus, in the second place, we have a vast dynamic system. We observe and study in the conditioned reflexes of our normal dogs this continual systematization of the processes, this, one may say, constant tendency towards a dynamic stereotype. Here is a most illustrative fact. If we elaborate in an animal a number of conditioned positive as well as inhibitory reflexes from stimuli of different intensity, and apply them during a certain period of time from day to day at regular intervals between the stimuli and always in a definite order, we establish thereby a stereotype of processes in the cerebral hemispheres. This can be easily demonstrated. If we now repeatedly apply throughout the experiment at equal intervals only one of the positive conditioned stimuli (better, one of the weak stimuli), it will reproduce in the proper sequence the fluctuations in the strength of the effects, as they were represented by the entire system of the various acting stimuli.

Not only the establishment, but a more or less lasting maintenance of the dynamic stereotype, is a nervous task of considerable difficulty, the degree of which depends on the complexity of the stereotype and on the individuality of the animal. There are, of course, nervous tasks the solution of which requires even from animals of the strong nervous type painful efforts. Other animals react to any simple change in the system of conditioned reflexes, such as the introduction of a new stimulus, or even to a certain transposition of the old stimuli, by complete loss of the conditioned reflex activity, sometimes lasting for a considerable period. Some animals can retain the proper system only if there are recesses in the experiments, i.e., if they are allowed certain rest. And finally, some animals show regular work only under a very simplified system of reflexes, consisting, for example, of two stimuli, both of them positive and of equal intensity.

It can be assumed that the *nervous processes in the cerebral hemispheres, when establishing and maintaining a dynamic stereotype,*[73] are what we usually call *senses* in their two categories—positive and negative, and their extensive gradation of intensity. The processes of establishing a stereotype, of fully accomplishing it, of its maintenance and derangement are subjectively different positive and negative senses, and that has always been manifested in the motor reactions of the animals.

Our entire work gradually enabled us to establish various types of nervous system in our animals. Since the cerebral hemispheres are the most reactive and supreme part of the central nervous system, their individual properties, naturally, must determine to a great extent the principal nature of the general activity of each animal. Our systematization of types coincides with the ancient classification of the so-called temperaments. There is the type with a strong excitatory process, but a relatively weak inhibitory process. Animals belonging to this type are aggressive and unrestrained. We call them strong and excitable or choleric. Next comes the type of strong and at the same time equilibrated animals, in which both processes are of equal strength. This is an easily disciplined and highly practical type which is met in two variations—quiet, sedate animals and active, lively ones. We name them respectively phlegmatic and sanguine. And finally, there is the weak inhibitable type, in which both processes are weak. We call such animals weak and also inhibitable since they are highly susceptible to external inhibition. They are cowardly and fussy and can be also characterized as melancholic, since everything constantly upsets them.

That our investigation of the higher nervous activity has taken the right road, and our definition of its phenomena, as well as our analysis of its mechanism are correct, is most convincingly proved by the fact that at present we are able in many cases to produce with great exactitude its functional chronic disturbances, and at the same time sub-

sequently to obtain a return to the normal at will. We know which type of our animals can be easily turned into neurotics, we know how to achieve this, and the kind of disorder that will set in. The strong, but unequilibrated, excitable and weak inhibitable types prove to be the best objects for the elaboration of experimental neuroses. If an excitable animal is persistently offered such tasks, the solution of which requires strong inhibition, then it loses it completely and is deprived of the ability to correct the conditioned reflexes, i.e., ceases to analyse, to distinguish the stimuli reaching it as well as the intervals of time. Stimulations produced by the strongest agents have no noxious pathological influence on them. With equal ease the weak inhibitable type becomes ill both under a slightly strained inhibition and under the action of very strong stimuli; it either fully loses its conditioned reflex activity under our experimental conditions, or manifests it in a chaotic way. As for the animals of the equilibrated type, we did not succeed in inducing nervous disorders in them even by colliding the opposite processes, which is a particularly morbific method.

Bromide proved to be the most reliable remedy against neuroses, just as it is in the human clinic; as shown by our numerous and in many respects instructive experiments, it has a special bearing on the inhibitory process, greatly tonifying it. However, very strict dosage is essential; for the weak type the dose of bromide must be from five to eight times smaller than that for the strong type. Rest, i.e., a recess in the experiments, often produces good results.

Among animals of the weak type there are frequent instances of natural neurotics.

We already have and we can even produce certain symptoms of psychotics: stereotypy, negativism and circularity.

Last year I specially acquainted myself with the clinic of human hysteria, which is regarded as being entirely or predominantly a mental disease, as a psychogenic reaction to the surroundings; as a result, I have become convinced that its symptomatology can, without any hesitation, be inter-

preted physiologically, from the point of view of the described physiology of the higher nervous activity, and I have expressed this conviction in the press.[74] However, some particulars of this symptomatology made us guess the existence of an addition which should be taken into consideration in order to get a general idea of the human nervous activity as well. This addition relates to the speech function, which signifies a new principle in the activity of the cerebral hemispheres. If our sensations and notions caused by the surrounding world are for us the first signals of reality, concrete signals, then speech, especially and primarily the kinesthetic stimuli which proceed from the speech organs to the cortex, constitute a second set of signals, the signals of signals. They represent an abstraction from reality and make possible the forming of generalizations; this constitutes our extra, *specially human, higher mentality* creating an empiricism general to all men and then, in the end, science, the instrument of the higher orientation of man in the surrounding world and in himself. The extreme fantasticism, the twilight states of hysterical persons, and the dreams of all men, are nothing more than the vitalization of the imaginative and concrete first signals, as well as of the emotions; the oncoming hypnotic state first of all switches off the organ of the system of the second signals—the most reactive part of the brain, which always predominantly functions in the wakeful state, and which regulates, and at the same time to a certain degree inhibits, both the first signals and emotional activity.

The frontal lobes, in all probability, represent the organ of this additional purely human mentality, but it can be assumed that it is subordinated to the same general laws of the higher nervous activity.

The foregoing facts, as well as the considerations based on them, are bound to lead to the closest connection between physiology and psychology—a development particularly observed in American psychology. In the 1931 Address of Walter Hunter, President of the American Psycho-

logical Association, despite strenuous efforts on the part of the speaker—who is a psychologist-behaviourist—to detach physiology from his psychology, it is absolutely impossible to see any difference between them. But even psychologists not belonging to the camp of behaviourists admit that our experiments with the conditioned reflexes have been of great help to the association theory of the psychologists. Other facts of a like nature could be cited.

I am convinced that an important stage in the development of human thought is approaching, a stage when the physiological and the psychological, the objective and the subjective, will really merge, when the painful contradiction between our mind and our body and their contraposition will either *actually* be solved or disappear in a natural way. Indeed, when the objective study of the higher animals, for example, the dog, reaches the level when the physiologist is able to foresee with absolute exactitude the behaviour of this animal under any conditions (and this level will be reached), then what will be left to prove the independent, separate existence of the subjective state, which the animal, of course, possesses but which is as peculiar as our own? When that occurs will not the activity of any living thing, man included, be indispensably regarded by us as a single, indivisible whole?

VI

THEORY OF ANALYSERS, LOCALIZATION OF FUNCTIONS AND MECHANISM OF VOLUNTARY MOVEMENTS

SUMMARY OF RESULTS OF THE EXPERIMENTS WITH EXTIRPATION OF DIFFERENT PARTS OF THE CEREBRAL HEMISPHERES BY THE METHOD OF CONDITIONED REFLEXES[75]

When I was confronted with the question of a subject for my report today I was uncertain for a time what to do—whether to take a small part of the subject, to review and discuss the results of a single series of experiments, or to make a general review of a considerable part of our work. I chose the latter course. It seems to me that a general review will be more instructive for my audience, and at the same time quite useful for ourselves. It is always of great value to review and summarize the work carried out over a period of years, to weigh its results, thoroughly to consider them, to define more distinctly our shortcomings and to fix our goal and tasks for the future.

In my laboratory we have been occupied for seven years now with the partial and complete extirpation of the cerebral hemispheres; scores of dogs have been used for this purpose, and this has provided ample data which must be thoroughly summarized. To this I shall now proceed.

As most of the audience knows, we, many years ago, expressed our special point of view regarding the higher nervous activity, as manifested in the higher animals. In the study of this activity we rejected the subjective, psychological conceptions and chose the external, objective point of view—the method employed by naturalists in studying

the material of their sciences. From this point of view the entire complex nervous system, previously interpreted as psychical, appears to us as the expression of two chief mechanisms—the mechanism of the formation of temporary connections between the agents of the external world and the activities of the organism, i.e., according to our usual terminology, the mechanism of conditioned reflexes, and the mechanism of the analysers, i.e., of an apparatus whose purpose is to analyse the complexity of the external world, to decompose it into separate elements and moments. At least until now all the results obtained by us fit into these concepts. This, however, does not exclude the possibility of a further extension of our concepts relative to this subject.

As the audience is also aware, the study of the complex nervous activity is carried out by us on an organ of minor physiological importance—the salivary gland; nevertheless, the two mechanisms governing the work of the cerebral hemispheres, which I have mentioned above, are very clearly manifested in the activity of this organ.

I shall, naturally, submit my material not in chronological order, that is, not in the order in which the facts were obtained by us, but in their logical sequence, arranging the material in such a way as to clarify the essence of the matter.

The first question to be decided here is the relation of the cerebral hemispheres to the above-mentioned mechanisms—to the mechanism of the formation of conditioned reflexes and to the mechanism of the analysers. The fundamental fact which in the course of the seven years was constantly observed and established by us and our numerous colleagues in a large number of animals is that the cerebral hemispheres are the seat of conditioned temporary reflexes, that one of the most important functions of the cerebral hemispheres consists precisely in the formation of conditioned reflexes, of temporary connections. We have very many facts testifying to this, although, of course, our subject is of such a character that every additional proof is

always helpful. When fully extirpating the cerebral hemispheres, or removing certain parts of them, the experimenters observed the disappearance either of all the conditioned reflexes or only of certain groups of them. Various measures were taken to obtain the most precise and pure facts, and the results were always the same. Under certain conditions all the conditioned reflexes, or only some of them, invariably disappeared. We were most persevering in this experimental work; in some cases we tried for years to restore a reflex before deciding that it was impossible. In the case of one dog, we even went so far as to accompany the feeding—not only in the experimental chamber, but at all times—with a certain sound, by means of which we expected finally to form, if it was at all possible to do so, a conditioned reflex. However, since the organ of the given conditioned stimulus was destroyed, the reflex could not be formed. In view of these, so to speak, stubborn facts, it had to be admitted that the cerebral hemispheres are, in effect, the organ of temporary connections, the birthplace of the conditioned reflexes. Certainly, one might categorically ask the following question: can these conditioned, temporary connections be formed also outside the cerebral hemispheres? But it seems to me that there are no grounds for considering this question. The facts already obtained by us inevitably lead to the conclusion that the temporary connections owe their emergence to the cerebral hemispheres and that they disappear with the extirpation of the latter. But, of course, it is possible that sometimes, in certain specific conditions, conditioned reflexes may arise also outside the cerebral hemispheres, in another part of the brain. In this respect one cannot be too rigid, since all our classifications and laws are always of a more or less conditional character and are valid only for the given time, under the given methods and within the limits of the given available material. Still fresh in all our minds is the recent example—the indivisibility of the chemical elements, long regarded as a scientific axiom.

Thus, I repeat, in the course of various experiments many investigators constantly found that the temporary connections developed only in the presence of the whole or part of the cerebral hemispheres. In view of this, we can now admit without any hesitation that one of the essential functions of the cerebral hemispheres is precisely the elaboration of conditioned reflexes, just as the most important function of the lower parts of the nervous system is connected with the simple reflexes, or in our terminology, unconditioned constant reflexes.

The second mechanism related to the cerebral hemispheres is the mechanism of the so-called analysers. In this respect we proceeded from the old facts, but somewhat modified their interpretation. We define an analyser as an apparatus whose purpose is to decompose the complexity of the external world into separate elements; for example, the eye analyser consists of the peripheral part—the retina, of the optic nerve and, finally, of the cerebral cells in which this nerve ends. The union of all these parts into a single mechanism, which is called analyser, is justified by the fact that so far physiology does not possess any data for an exact division of the entire analysing activity. So far we cannot say which part of it is performed by the peripheral section and which by the central one.

Thus, the cerebral hemispheres, according to our understanding, consist of a number of analysers—the eye, ear, skin, nose and mouth analysers. Study of these analysers led us to the conclusion that their number must be increased, that in addition to the above-mentioned analysers relating to the external world, the existence of special analysers in the cerebral hemispheres must be recognized, whose function is to decompose the enormous complexity of the internal phenomena arising within the organism itself. Undoubtedly, not only an analysis of the external world is of importance to the organism; it also needs a signalling upwards and an analysis of everything taking place inside the organism itself. In a word, in addition to the external

analysers already mentioned, there must be internal analysers, the most important of which is the motor analyser, the analyser of movement. We know that from all parts of the motor apparatus—from the joint capsules and surfaces of the joints, tendons, etc., there stretch centripetal nerves which signalize every moment the slightest detail of the act of movement. All these nerves unite at the supreme points—in the cerebral cells. The various peripheral endings of these nerves, the nerves themselves, as well as the nerve cells, in which they end, in the cerebral hemispheres, constitute a special analyser which decomposes the motor act with its enormous complexity into a large number of the most delicate elements; this ensures the enormous variety and the precision of our skeletal movements.

The concept of such an analyser is of particular interest in the physiology of the cerebral hemispheres. As you know, in 1870 (the year when fruitful scientific study of the cerebral hemispheres began) the Germans Fritsch and Hitzig demonstrated that stimulation of definite parts of the cortex in the anterior half of the cerebral hemispheres by means of an electric current evoked a contraction of certain groups of muscles. This discovery supplied the grounds for recognizing the existence of special motor centres in these places. But then the question arose as to how these parts of the cerebral hemispheres should be pictured. Are they motor centres in the full sense of the term, i.e., cells from which impulses proceed direct to the muscles, or are they sensory cells to which the peripheral stimulations come and from which the latter are merely transmitted to the active motor centres, the motor cells, where the motor nerves going direct to the muscles originate? This controversy, started by Schiff, is still in progress.

We also had to take part in deciding this question and this is how we did it. We had long inclined to the view that the places in the cerebral cortex, stimulation of which results in certain movements, represent aggregations of sensory cells, or brain endings of the centripetal nerves

going from the motor apparatus. But how to obtain more or less convincing proofs of the correctness of this view? In addition to the old-established facts already utilized by the adherents of this view, we succeeded in finding fresh proof which, in our opinion, is most convincing.

If the so-called motor region is really the motor analyser, fully analogous to any other analyser—the ear, the eye, etc.—then the stimulation brought to this analyser can be directed along any centrifugal path, i.e., the stimulation can, at our will, be connected with any activity. In other words, in this case a conditioned reflex can be elaborated from a motor act. And we did elaborate it. Dr. Krasnogorsky, applying, on the one hand, our usual stimuli, for example, acid, and, on the other hand, flexing a certain joint, formed a conditioned reflex, a temporary connection between the flexion and the work of the salivary gland. Definite movements produced the same secretion of saliva as was the case with conditioned stimuli from the eye, ear, etc. Then the question arose, how correct is the interpretation of this fact, and is it really a reflex proceeding from flexion, in other words, from the motor act, or a reflex from the skin? In this respect, too, Dr. Krasnogorsky was fortunate enough to round off his proof, one can say, to the point where it was beyond reproach. When he formed a cutaneous reflex on one of the legs of the dog, and a flexion reflex on another, and then extirpated different parts of the cerebral hemispheres, the following phenomena were observed. If the g. sigmoideus was removed, the flexion reflex disappeared, but the cutaneous reflex persisted and could be elaborated. On the contrary, when the gg. coronarius and ectosylvius[76] were removed, the cutaneous reflex disappeared, while the flexion reflex remained. Thus, it was established beyond doubt that the cutaneous and motor analysers are different and that the motor analyser is located in the motor region of the brain.

It seems to me that all these experiments give us the right scientifically to speak of the motor analyser in exactly

the same way as we do in respect of the eye, ear and other analysers.

It remains for us to explain why movement is provoked by electrical stimulation of those areas in the cerebral hemispheres where, as some investigators believe, the special motor centres are located. Since, in our opinion, it is the sensory cells of the motor analyser that are located here, and consequently, from here throughout lifetime stimulations normally and constantly stream out to definite motor centres, it is clear that with such well-beaten paths and with electrical stimulation of these areas, a usual effect arises, i.e., the stimulation proceeds from here along the customary path to the muscles.

Thus, on the basis of all our experiments we can say that the cerebral hemispheres constitute a combination of analysers, having the function, on the one hand, of analysing the external world, for example, the eye and ear analysers, and on the other hand, of analysing the internal phenomena, for example, the motor analyser. As for all the possible internal analysers, it is clear that the analysis of any other internal phenomena must be much more limited. So far, apart from the motor analyser, no other analysers of this kind have been revealed by the method of conditioned reflexes. There is no doubt, however, that sooner or later, these phenomena, too, will enter into the physiology of the conditioned reflexes.

Now let us pass to a thorough consideration of the analysers. What functions do they perform? As indicated by their name, their purpose is to decompose complex phenomena into separate elements. But what else do we know about their functions, and what have our experiments, based on the method of conditioned reflexes, shown us in this respect? I think that here the objective point of view has been of great service to us. The general facts relating to the work of the analysers have been known for a long time. The research carried out by Ferrier and Munk provided a number of facts which have a bearing on the

work of the analysers. But these facts were elucidated from a very confused and unscientific point of view. You probably remember that when Munk extirpated the occipital and temporal lobes of the cerebral hemispheres, he observed certain abnormalities of hearing and sight in the dog subjected to the operation. He termed the peculiar attitude of the animal towards the external world which resulted from these abnormalities of hearing and sight, "psychical deafness" and "psychical blindness." But what did this mean? Let us consider psychical blindness. This meant that after the removal of the occipital lobes the dog did not lose the ability to see; it avoided objects met on its way, distinguished between light and dark, but at the same time no longer recognized its master whom formerly it had known very well; it completely failed to react to him; if he existed at all for the dog, it was only as an optical stimulus. The dog displayed the same attitude to all other objects. Munk and others assert that the dog "sees" but "does not understand." But what does this mean—he "understands" and "does not understand"? These words express nothing definite; they, too, must be explained.

Only the method of conditioned reflexes, excluding all psychological concepts ensured a solid foundation to the matter and fully clarified it. From the objective point of view the destruction of a certain part of the cerebral hemispheres was regarded as complete removal or partial destruction of one or another analyser. If the given analyser remained intact and its cerebral end undamaged, the dog, by means of this analyser, could differentiate both separate elementary phenomena and their definite combinations, i.e., it behaved normally. But when the analyser was destroyed, or damaged to a greater or lesser degree, then the dog could no longer differentiate delicately the corresponding phenomena of the external world. And the more the analyser is destroyed, the greater the decline of analysis. If the analyser is completely destroyed no trace remains of analysis even of the simplest phenomena. If, however, some

fragments of the analyser remain, if a certain part has escaped destruction, then the correlation between the organism and the environment in respect of the given phenomena also remains, although in a very general form. Further, the larger the part of the analyser remaining intact, the more of it that remains uninjured, the better and the more delicate is the analysis it can effect. In brief, since the damage of the analyser is regarded as damage of a mechanism, it is clear that the greater the scale of the damage, the poorer is its capacity for work. This concept makes the subject quite clear and paves the way for further investigation, while the psychological point of view brings the subject into a blind alley and cannot add anything to the words "understands" and "does not understand."

Now let us consider Munk's experiments from our standpoint. We destroy the occipital lobes of the animal, i.e., the cerebral end of the eye analyser. If after this operation a minimal part of the analyser remains undamaged, the animal is still capable of a very crude analysis; it can distinguish only between light and dark. In such animals it is impossible to elaborate conditioned reflexes to the form of objects or to their movement. At the same time, however, a reflex to light or darkness is easily formed in them. If, for example, during the feeding of the animal you repeatedly produce an intensive light, then afterwards, as soon as the light appears, the animal begins to show a secretion of saliva; this means that only the small part of the analyser left after the extirpation of the occipital lobes continues to function. This explains why Munk's dog did not stumble against objects in its way; it could distinguish between dark and light, and thus avoided objects. In this limited way the eye analyser functioned very well. But when a more delicate analysis was required, when it was necessary to distinguish between various combinations of light and shade, as well as between different forms, the power of analysis proved insufficient and the damaged analyser did not function. It is understandable, therefore,

that a dog in this state cannot recognize its master—it cannot distinguish him from other objects. The point is absolutely clear, and there is no need for vague formulations. Instead of saying that the dog no longer understands, we say that its analyser is injured, with the result that it has lost the ability to form conditioned reflexes to more delicate and more complex visual stimuli. And now our big task is to investigate this analyser step by step, to study its action when fully intact and to see what disappears gradually from its activity when damaged to one or another degree.

We already have precise and convincing data in this respect. If after extirpation an insignificant part of the eye analyser of the dog is left, then in such an animal a conditioned reflex can be evoked only by intensity of light, and nothing else. If the analyser is injured to a lesser degree, a reflex can be elaborated also to the movement of an object, later to its form, etc., up to the point of normal activity.

The same is true for the ear analyser. If a small part of it is left undamaged or if its activity is temporarily inhibited to a similar degree, then the animal distinguishes only between silence and sound. For an animal in this state different sounds are identical—for it all sounds, noises and tones, both high and low, are the same; it reacts only to the intensity of the sounds, but does not discern their detailed properties. If the damage to the analyser is less and a larger part of it is left, then it is possible to form reflexes to noises and tones separately; this means that here we have also a qualitative analysis, although a crude one. When the damage is still less, a reflex can be formed to separate tones and different varieties can be observed: the less the injury, the more delicate is the analysis of tone. When the analyser is severely damaged, the animal distinguishes only between big intervals of pitch, for instance, octaves; if the damage is moderate, it distinguishes first between tone, and then between fractions of a tone (½, ¼ of a tone).

Thus there takes place a gradation from complete inability to analyse to perfectly normal activity of the ear analyser.

Now I shall dwell on the highly interesting experiments carried out by Dr. Babkin. One of his dogs lived for three years after extirpation of the posterior part of its cerebral hemispheres; thus it can be said that the condition of the dog became stationary. The dog distinguished perfectly not only between noise and sound, but also between different tones. To one tone there was a definite reflex, while to another—a close tone—no reflex at all, which shows that in this respect the dog was quite normal. But it suffered from an irreparable defect: it could not distinguish between more complex sound combinations. For example, you elaborate in the dog a conditioned stimulus from a series of ascending tones—*do, re, mi, fa*. After some time you obtain a corresponding conditioned reflex. Now you reverse the tone sequence to *fa, mi, re, do*. A normal dog distinguishes the change very well, but this animal is unable to make such an analysis, and the change means nothing to it; it cannot differentiate between the sequence of sounds. Try as you may, no differentiation will be obtained. The damage to the analyser is so great that the dog is unable to perform this work. Closely linked with this fact is another, old one, to which the words "understands" and "does not understand" have also been applied: it relates to dogs which, because of damage to the ear analyser, failed to respond to their names. The just mentioned dog was named "Ruslan," but after the operation the name, even if repeated a thousand times, produced no effect whatever. Obviously the ear analyser of this animal was in such a state that it could not distinguish one combination of sounds from another. If the dog is unable to distinguish between the group of tones *do, re, mi, fa* and the same group in a reverse order—*fa, mi, re, do*—then, of course, it cannot recognize its name, since the word "Ruslan" is an even more complex sound combination. Such an analysis is beyond the ability and power of its damaged ear analyser.

I wish to stress once more the great merit of the objective method, the method of conditioned reflexes, in studying the function of the analysers. This method has completely stripped all mystery from the subject, discarded the meaningless words "understands" and "does not understand," and has replaced them with a clear and effective programme for the study of the analysers.

The task of the investigator is to define exactly the functions of the analysing apparatus, to investigate all the variations in its operation in cases of destruction of its different parts. And from the mass of facts so obtained it will be possible to attempt to reproduce the structure of the analyser, to establish its parts and discover how these parts interact.

So much for the activity of the analysers. As for the topography of the analysers and their arrangement, it should be pointed out that the view concerning their exact localization, established on the basis of earlier facts, cannot be regarded as satisfactory. Even in the past many objections to this view had been raised. Our experiments have also shown that the formerly established limits of the analysers are incorrect, that actually they are much wider, not so distinctly separated, but intermingled and interwoven one with another. Of course, it is a very difficult matter to define exactly the localization of the analysers in the cerebral hemispheres and to establish how and why they are interlaced.

Thus, from the point of view of the conditioned reflexes the cerebral hemispheres appear as a complex of analysers, whose purpose is to decompose the complexity of the internal and external worlds into separate elements and moments, and then to connect all these with the manifold activity of the organism.

Now another question arises, closely connected with the method of conditioned salivary reflexes, a question which in all probability cannot be decided or even strictly formulated without this method. Here it is: is the activity of

the cerebral hemispheres confined to the mechanism of the formation of temporary connections and to the mechanism of the analysers, or are there not other, higher mechanisms which so far have not been designated by names? This is not a far-fetched question, but one which is advanced by life, by experimental practice. If you extirpate the entire posterior part of the cerebral hemispheres in a dog, i.e., just behind the gyrus sigmoideus and then along the fissura Sylvii, you will have a generally normal animal; it will recognize you and the food, as well as all other objects on its way, with the help of its nose and skin. It will wag its tail when you stroke it, evince joy upon recognizing you by sniffing, etc. But this animal will not react to you if you are at a distance, i.e., it does not use its sight in the normal manner. Nor will it react if you call it by name. You are bound to conclude that this dog uses its eyes and ears only very little, but in other respects it is absolutely normal.

But, if you remove the anterior part of the cerebral hemispheres along the same line as in the above-mentioned operation with the removal of the posterior part, you will, to all appearances, get a completely abnormal animal; its attitude to you, to other dogs, and to food (which it will be unable even to detect), and generally to all the surrounding objects, will be abnormal; it will be a completely mangled animal, obviously with no trace of proper behaviour left. Thus, there is a big difference between the two animals—the one without the anterior, and the other without the posterior part of the cerebral hemispheres. With regard to the first animal you would say that it is blind or deaf, but otherwise normal, about the other that it is a confirmed invalid and a helpless imbecile.

Such are the facts. An important and perfectly lawful question arises: Is there not something special in the anterior parts of the cerebral hemispheres, and are they not called upon to fulfil higher functions than the posterior parts?

Perhaps here, in the anterior parts, there is concentrated the most essential activity of the cerebral hemispheres?

I believe that the method of conditioned salivary reflexes provides a clear answer to this question, an answer that cannot be obtained by any other method of investigation. Is it really the case that an animal with the anterior parts of the cerebral hemispheres extirpated differs essentially from a normal animal and shows no trace of normal higher nervous activity? If you adhere to the old methods of investigation, if you observe only the work of the skeletal muscles, then you will reply to this question in the affirmative. But if you turn to the salivary gland with its conditioned reflexes, the picture will be entirely different. This is not only the merit of the method of conditioned reflexes; it is also due to the fact that precisely the salivary gland was selected for the study of these reflexes. If you observe the work of the salivary gland in such an animal, which at first sight seems completely disabled, you will be surprised at the high degree the gland maintains its complex nervous relations. You will not observe even the slightest disorder in the function of the gland. On the basis of this gland you can elaborate in such an animal temporary connections, inhibit and disinhibit them, etc. In short, the salivary gland displays the whole complex of the relations observed in the normal animal. You can see clearly an unexpected discrepancy between the work of the skeletal musculature and that of the salivary gland. While the work of the former is abnormal and deranged, the latter functions perfectly well.

What does all this mean? First of all it is quite obvious that there are no mechanisms in the anterior lobes dominating the entire cerebral hemispheres. If such mechanisms existed, then the removal of the anterior lobes would destroy the entire delicate and complex work of the salivary gland. However, everything proceeds here quite normally. Evidently, we must admit that all the abnormalities which we observe in this dog are phenomena which relate only

to the skeletal muscles. And our task boils down to revealing the causes of this disturbance in the work of the skeletal muscles. The existence of any general mechanism in the anterior lobes is out of the question. Obviously, the latter do not contain any particularly vital arrangement that could be regarded as establishing the highest perfection of the nervous activity.

Here is a simple explanation of this peculiar disturbance in the work of the skeletal musculature. At any given moment this work greatly depends on the cutaneous analyser and on the motor analyser. Thanks to these the animal's movements are constantly co-ordinated and adapted to the surrounding world. Since in the given dog both the cutaneous and the motor analyser are destroyed, the general activity of its skeletal musculature is, naturally, profoundly disturbed. Consequently, when the anterior lobes are destroyed we actually get a partial defect, just as in the case of the damage to the eye analyser, but not a general defect which might ensue from the abolition of the work of a hypothetical higher mechanism of the cerebral hemispheres situated in the anterior lobes.

In view of the importance of this question, a series of corresponding experiments were carried out by Drs. V. A. Demidov, N. M. Saturnov, and S. P. Kurayev. The experiments were so conducted that first the entire anterior parts together with the olfactory lobes were removed from the dog. In this dog it proved possible to elaborate a conditioned salivary reflex only from the mouth cavity by using water; when acid was repeatedly poured into the dog's mouth as an unconditioned stimulus of the salivary gland, then the subsequent introduction of water, which formerly had been absolutely indifferent to the gland, also caused a secretion of saliva, thus acting as a conditioned stimulus. However, since this water reflex might be regarded as being doubtful, it was necessary to prove the existence in such a dog of other conditioned reflexes. Therefore, Dr. Saturnov extirpated the anterior lobes but left the olfactory ones.

Then, after the operation, it was possible to obtain in this dog a conditioned reflex from the olfactory nerves.

After these experiments we regarded the subject as being sufficiently clarified, and we reached the final conclusion that a dog deprived of the anterior parts of the cerebral hemispheres loses only particular mechanisms, i.e., some of the analysers, but by no means any general mechanisms.

Thus, the study of the activity of the cerebral hemispheres by the method of conditioned reflexes provides an absolutely definite answer. Basing ourselves on precise facts, we can state that the cerebral hemispheres represent a system of analysers, which decompose the complexity of the external and internal worlds into separate elements and moments and then connect the phenomena thus analysed with one or another activity of the organism.

Can we be satisfied with the results obtained? Of course, we can, and chiefly because they have paved the way for further fruitful study of the subject. At the same time, however, it is clear that this study has just been started, and that the most complicated, the most essential part of this study is still ahead of us. In outlining the further course of research, we must pay attention first of all to our present method of dismembering the apparatus under investigation into separate parts. It is an awful method! The more we experiment with extirpation of the cerebral hemispheres, the more we are surprised at the successful results achieved by former investigators by means of this method. Because of extirpation we hardly ever obtain stable conditions; they bear a fluctuating, changeable character. You place your heavy hands on the brain, and damage it by removing certain of its parts. The damage irritates the brain and this irritation persists for some time, spreading to uncertain limits. You never know when its action will end. That such irritation really exists is proved by many well-known experiments on which I shall not dwell here. But finally the desired moment comes—the irritation evoked by the damage passes, and the wound begins to heal. Then a new irritation

appears—the scar. And it may be that there are only a few days at your disposal during which you can work with the assurance that all the changes observed depend only upon the absence of the removed parts of the cerebrum. Then the following phenomena begin to develop. At first the phenomena of depression appear, and you know that it follows from the action of the scar. This state lasts for days, and then convulsions set in. After this, after the excitation, there comes a new period of subsequent depression, or an entirely new, peculiar state of the animal develops. After the convulsions a drastic change takes place in the dog and you cannot recognize it: it becomes much more disturbed than was the case immediately after the operation. Obviously the scar not only irritates, but exerts pressure on the tissues, strains and wrenches them, i.e., causes fresh damage.

I must add that this effect of the scar never ends, at least I have never seen it end. Sometimes it persists for months and years. Convulsions usually appear after a month or a month and a half, and then recur. We have operated on scores and scores of dogs, and I can state categorically that not one of them escaped convulsions and recurring fits of them provided it survived the first attack.

Just try under these unfavourable conditions to analyse successfully such complex activity as that of the cerebral hemispheres. Without a shadow of doubt the present-day investigator of the cerebral hemispheres must, above all, attend to the question of how to adapt the whole procedure of his investigation to the brain. This is a highly important matter, since the present method of investigation entails a tremendous waste of human labour and a great loss of animals. Endeavours have already been made to reduce this waste. A German experimenter (Trendelenburg) has attempted local cooling of the brain. In our laboratory this method is employed by Dr. L. A. Orbeli. The near future will show whether this method will be suitable and bring us good results.

Such are our achievements, our aspirations, our complaints and hopes.

PHYSIOLOGICAL MECHANISM OF THE SO-CALLED VOLUNTARY MOVEMENTS[77]

In the physiological laboratory of the Military Medical Academy Dr. Krasnogorsky (1911) definitely established the undoubtedly afferent nature of the motor region of the cortex by forming from the kinesthetic stimulation[78] of the skeletal musculature a conditioned food stimulus just as it is formed from all other stimulations entering the cortex through the external receptors—the eye, the ear, etc. In other words, he showed that any passive skeletal movement could be made a signal of a positive unconditioned alimentary reflex, that is, a conditioned alimentary stimulus. Y. M. Konorsky and S. M. Miller, who had obtained their basic experimental facts in Warsaw and who are going ahead with their elaboration in the Physiological Department of the Institute of Experimental Medicine, afterwards applied successfully kinesthetic stimulation (passive movements) as signals of unconditioned negative reflexes (a painful stimulation of the ear, the introduction of acids, etc.) and as conditioned inhibitors for both groups of unconditioned reflexes. In this way many facts were obtained relating to the highest important physiological problem of the mechanism of voluntary movements, i.e., movements proceeding from the cerebral cortex.

First of all we must recognize as an established fact that a definite movement corresponds to the stimulation of definite kinesthetic cells in the cortex, and, on the contrary, a passive reproduction of a definite movement, in its turn,

evokes impulses in the kinesthetic cerebral cells, the stimulation of which actively produces this movement. This can be proved in the following way. The first part of the above proposition is a constant physiological fact of long standing: when definite points of the surface of the motor region in the cerebral cortex are stimulated by means of a weak electric current, mechanically or chemically, strictly definite skeletal movements arise. As for the second part of the proposition, it is proved by the simplest facts relating to the training of domestic animals, for example, dogs. You lift the dog's paw saying "give me the paw," or simply "paw," and then give the dog something to eat. After repetition of this procedure the dog itself offers its paw at these words; it does so even without any word of command when it has a keen appetite, i.e., when it experiences alimentary excitation. The physiological conclusions from this well-known and constant fact are both obvious and many-sided. It is clear, in the first place, that the kinesthetic cell, stimulated by a definite passive movement, produces the same movement when it is stimulated not from the periphery, but from the centre; in the second place, that the kinesthetic cell establishes a connection both with the auditory cell and the cell of alimentary excitation, or the gustatory cell, since the stimulations coming from both of these cells evoke an active state of the kinesthetic cell; and in the third place, that in this interconnected system of cells the process of excitation moves in two opposite directions—from the kinesthetic cell to the gustatory, alimentary one (during the establishment of the connection) and from the alimentary to the kinesthetic cell (in the case of alimentary excitation). These conclusions are also confirmed by other facts. It has long been observed and scientifically proved that when you think of a certain movement (i.e., when you have a kinesthetic idea), you involuntarily, without noticing it, produce the movement. The same thing occurs in the well-known trick when a man has to do something of which he has no knowledge: he has to go somewhere and do something with the

help of another man who knows how to do the thing, but has neither the intention nor the desire to help. However, to get actual help it suffices for the first man to take the other's hand in his own. In this case the second man involuntarily, without noticing it, gives the first slight pushes towards the goal and keeps him from moving in the opposite direction.[79] The process of learning to play the piano or the violin from music entails an obvious transition of the excitation from the visual to the kinesthetic cell.

Thus, the kinesthetic cells of the cerebral cortex can, and do establish connections with all the cerebral cells which represent the external influences as well as various internal processes of the organism. It is this that constitutes the physiological basis of the so-called voluntariness of movements, i.e., of their dependence on the aggregate activity of the cortex.

Still unsolved in this physiological concept of voluntary movements is the question of the cerebral connection of the kinesthetic cells with the corresponding motor cells, whence the pyramidal efferent paths originate. Is this connection inborn, or is it acquired, elaborated in the course of the post-natal existence? Most probably the latter is the case. If this connection is constantly extended and perfected during the entire lifetime, then it can be assumed that even the first period of the individual existence of higher animals, and especially of man who for months learns to control his first movements, is spent on establishing this connection.

The general physiological law of the work of the skeletal musculature, on the one hand, consists in constant movement towards everything, in grasping everything that preserves and ensures the integrity of the animal organism, that equilibrates it with the surrounding medium; this is a positive reaction, positive movement. On the other hand, it is constant movement from everything, throwing aside and ejecting everything that hinders and threatens the vital process, that violates the equilibration of the organism with

the environment; this is a negative reaction, negative movement. A conditioned stimulus is a signal, as it were, a substitute for the unconditioned stimulus. This explains why a dog, for example, reaches for the electric bulb and even licks it, when its flashing acts as a conditioned alimentary stimulus. And on the contrary, under the action of a conditioned acid stimulus, the dog reproduces the movements it makes when acid is introduced into its mouth. The same thing occurs when a kinesthetic stimulation acts as a conditioned stimulus. Thus a passive movement, when it is connected with an alimentary reflex, evokes a positive alimentary reaction, and when it is connected with an acid reflex—a negative, acid reaction.

Now let us consider all the cases in which kinesthetic stimulation (passive movement) has been applied by the researchers in studying conditioned reflex activity.

1. When the flexion of a leg is connected with an alimentary reflex, it is reproduced by the animal, just as any other natural alimentary movement, every time it is in a state of alimentary excitation; this takes place so long as the connection functions, so long as it is not eliminated by a protracted non-reinforcement, or is not abolished temporarily by one or another kind of inhibition.

2. In the case of a conditioned acid reflex, when the flexion of the leg is a signal, a substitute for the acid, a struggle begins against the flexion, just as against the acid itself. The flexion must be eliminated just as the acid must be ejected from the mouth. But flexion can be eliminated by extension, and it is the latter phenomenon which is actually observed. It is a well-known fact that when for some reason the flexion is associated with pain, the animal keeps its leg extended.

3. In the case of flexion, applied as a conditioned inhibitor, i.e., when a passive movement is added to the conditioned alimentary stimulus, but no food is offered, this movement is a signal of the animal's difficult state caused by inducing but not satisfying the alimentary excitation.

Naturally, there must be a struggle against it: it must be eliminated—and this is attained by extension.

4. In the last case, when the flexion of the leg is added to the conditioned acid stimulus as a conditioned inhibitor, without introducing acid, the passive movement is a signal of the elimination of the noxious agent; at the same time it is, as it were, a reliable means of combating this agent, and is, naturally, always repeated by the animal even when it encounters other noxious agents.

But all that has been said above explains the phenomena only from the more general physiological point of view. It is impossible not to see that the mechanism of some particular physiological phenomena still remains to be elucidated. The question arises: how and on what immediate basis does the transition from flexion to extension take place, since physiologically these motor acts are definitely and constantly interconnected? And another question: was there manifested in the third and fourth cases, and if so when and how, the inhibitory process which, in our experiments, inevitably appeared when the combination of a conditioned stimulus with an extraneous one was not reinforced by a corresponding unconditioned stimulus? These questions must be subjected to further experimental analysis, since the available data are insufficient to answer them properly.

VII

THEORY OF TYPES

GENERAL TYPES OF ANIMAL AND HUMAN HIGHER NERVOUS ACTIVITY[80]

The mode and standards of our own behaviour, as well as of the behaviour of the higher animals close to us and with which we are in constant vital relations (for instance, dogs), represent a great, a truly boundless variety, if behaviour is considered as a whole, in its smallest details, especially as manifested in man. But since our behaviour, as well as that of higher animals, is determined and controlled by the nervous system, it is possible to reduce the above-mentioned variety to a more or less limited number of b sic properties of this system, with their combinations and gradations. This makes it possible to distinguish between the types of nervous activity, i.e., between these or other complexes of the basic properties of the nervous system.

The observation and study of a large number of dogs, using the method of conditioned reflexes, carried out in our laboratory for many years, have gradually disclosed to us these properties in their vital manifestations and combinations. These properties include: in the first place, the *strength* of the basic nervous processes—excitatory and inhibitory—which always constitute the sum total of nervous activity; in the second place, the *equilibrium* of these processes; and, finally, in the third place, their *mobility*. It is obvious that while all these properties exist and act simultaneously, they provide the highest adaptation of the animal's organism to the surrounding world, or, in other words, the complete equilibration of the organism as a whole with

the external environment, i.e., they secure the organism's existence. The significance of the strength of the nervous processes is clearly shown by the fact that in the surrounding medium there arise (more or less often) unusual, extraordinary developments, powerful stimuli, and that, naturally, other external conditions of a similar and even greater force not infrequently necessitate the suppression or retardation of the effects of these stimuli. And the nervous cells must endure this extraordinary tension in their activity. From this also follows the importance of equilibrium between both processes, their equal strength. Since the organism's external environment is constantly—and often powerfully and abruptly—fluctuating, both processes must, so to speak, keep pace with these fluctuations, i.e., they must possess great mobility and be able, in compliance with the demands of the external conditions, rapidly to recede, to give preference to one stimulus, to excitation before inhibition and vice versa.

Leaving aside the gradations and considering only the extreme cases, only the limits of fluctuation, viz., strength and weakness, equality and inequality, lability and inertness in both processes, we obtain eight combinations, eight different complexes of basic properties of the nervous system, eight types of the nervous system. If we also take into account that in the absence of equilibrium the predominance may, generally speaking, be on the side now of the excitatory, now of the inhibitory process, and that in the case of mobility, inertness or lability may also become a property now of one, now of the other process, then the number of possible combinations increases to twenty-four. And finally, if we also take into consideration even the rough gradations of the three basic properties, we shall thereby again greatly augment the number of possible combinations. However, only extensive and thorough observation can establish the presence, frequency and intensity of these or other actual complexes of basic properties, of the actual types of nervous activity.

Since normally our general behaviour, as well as that of higher animals (we imply here healthy organisms), is directed by the higher part of the central nervous system—by the cerebral hemispheres and the adjacent subcortex—the study of this higher nervous activity under normal conditions by the method of conditioned reflexes is bound to lead to knowledge of the actual types of nervous activity and the basic standards of behaviour of human beings and higher animals.

It seems to me that this problem was solved—of course, only in general outline—by the Greek genius in his system of the so-called temperaments, where the basic components of the behaviour of human beings and higher animals were exactly emphasized and advanced, as we shall show in our further exposition.[81]

But before proceeding to our factual material, I must touch on one very substantial and so far almost insurmountable difficulty connected with the definition of the type of nervous activity. Human and animal behaviour is determined not only by congenital properties of the nervous system, but also by the influences to which the organism is continuously subjected during its individual existence; in other words, it depends on constant education and training in the broadest sense of these words. This is due to the fact that along with the above-mentioned properties of the nervous system, another very important property incessantly manifests itself—its high plasticity. Consequently, since this is a question of the natural type of nervous system, we must take into account all the influences to which the organism has been exposed from the day of its birth to the present moment. With regard to our experimental material (i.e., our dogs) in the overwhelming majority of cases the fulfilment of this requirement still remains a passionate desire. We shall be able to fulfil it only when our dogs are born and reared before our eyes, under our unremitting observation. We shall soon have convincing corroboration of the importance of this requirement. So far there is only one way

of overcoming the above-mentioned difficulty: it is necessary to increase and to diversify the forms of our diagnostic tests as much as possible in the hope that in this or that case we shall succeed in bringing to light the specific changes in the natural type of nervous system that were determined by the definite influences of the individual existence; in other words, by means of a comparison with all other features of the type we shall reveal both the more or less disguised natural features and the elaborated, acquired ones.

Right from the very beginning of our experiments with dogs based on the method of conditioned reflexes we (like others) were struck by the different behaviour of the bold and the cowardly dogs. The former offered no resistance when led to experimentation; they remained quiet in the new experimental conditions, both when they were placed in the stands mounted on tables, and when certain apparatuses were attached to their skin and even placed in their mouths. When food was given to them by means of an automatic device, they began to eat it at once. Such was the behaviour of bold animals. But the cowardly animals had to be accustomed gradually to the procedure—a process which required days and even weeks. Another difference was observed when we began to elaborate conditioned reflexes in these dogs. In the first case the conditioned reflexes developed rapidly, after the application of two or three combinations; they reached considerable strength and remained constant, no matter how complicated the system of reflexes. In the second case, on the contrary, the conditioned reflexes were formed very slowly, after many repetitions; their strength increased at a very low rate, and they never acquired stability, being sometimes even at zero, no matter how considerably their system was simplified. It was, therefore, natural to assume that in the first dogs the excitatory process was strong, while in the second it was weak. In the bold dogs the excitatory process, which from the biological point of view arises properly and in time, for instance, at the sight of food, constantly resists minor influences, re-

maining, so to speak, legitimately predominant. In the cowardly dogs the strength of the excitatory process is insufficient to overcome conditions which are less important in the given case and which produce what we term external inhibition; for this reason we say that such dogs are inhibitable. In the bold dogs even physically excessive external stimuli, when conditionally connected with physiologically important functions, continue to serve their purpose without bringing the nerve cell to a pathological state; thus they represent an exact index of the intensity of their excitatory process, of the strength (i.e., working capacity) of their nerve cells.

It is here that the specific difficulty, which I have just mentioned made itself felt. All the dogs which seemed to us cowardly, i.e., which only very slowly became accustomed to our experimental conditions and formed conditioned reflexes with difficulty (since their entire conditioned reflex activity was easily disturbed by insignificant new external influences), were regarded by us, quite groundlessly, as belonging to the weak type of nervous system. This even resulted in a blunder—at one time I regarded these dogs as experts in inhibition, i.e., as being strong in this respect. The first doubts as to the correctness of this diagnosis arose in connection with the external behaviour of these animals in their habitual surroundings. Further, it seemed strange that their conditioned reflex activity, despite its high complexity, should be of a perfectly regular character so long as the surrounding conditions remained strictly uniform. But the final solution was found thanks to a special investigation. We (Virzhikovsky and Mayorov) took a litter of puppies and divided it into two parts: half of the puppies, from the very day of their birth, were kept in the kennel, the others were given complete freedom. All the animals of the first group turned out to be cowardly and susceptible to inhibition given the slightest changes in the surroundings; in the animals of the second group nothing of the kind was observed. It became clear that when the puppies first appeared in the ex-

ternal environment they were provided with a special reflex, sometimes referred to as a panic reflex, but which I suggest should be termed an initial and temporary reflex of natural caution. The moment acquaintance with the new environment begins it is necessary to wait some time for the consequences of any new stimulation, no matter which receptor it affects, i.e., to abstain from any new movement and to repress the existing movement, since it is not known what the new phenomenon promises the organism, whether harmful, useful, or of no consequence at all. And only in the course of the gradual acquaintance with the environment is this reflex replaced, little by little, by a new, special, investigatory reflex, and, depending on its effect, by other corresponding reflexes. The puppy, which is not given the opportunity to gain this practical experience independently, retains the persisting temporary reflex for a very long time, if not for life, and the reflex constantly disguises the real force of the nervous system. What a vital pedagogical fact this is! A sure sign of this unduly persisting feature, apart from the fact that in many respects it contradicts other stable inborn features, is the inhibitory action not so much of the particularly strong stimulations but of the new stimulations—no matter how weak they may be in themselves (Rosenthal, Petrova).

Thus, the strength of the excitatory process was regarded by us as the first property of the type of nervous system. Hence the initial division of all our dogs into strong and weak ones.

Another property of the nervous system, clearly observed by us and according to which the animals are subdivided into new groups, is the equality or inequality of the two opposite nervous processes—excitation and inhibition. We imply here the higher active cortical inhibition (or according to the terminology used in the theory of conditioned reflexes —internal inhibition), which, together with the excitatory process, continuously maintains the equilibration of the organism with the surrounding medium and helps (on the

basis of the analysing function of the organism's receptors) to distinguish between the nervous activity corresponding to the given conditions and moments and that which does not (extinction, differentiation and retardation).

The significance of this property was first observed by us in dogs with a very strong excitatory process. We soon noticed that whereas in such dogs positive conditioned reflexes were formed rapidly, inhibitory reflexes, on the contrary, were elaborated very slowly, with obvious difficulty; this was often accompanied by a violent resistance on the part of the animal; it was manifested either in destructive actions and barking, or, on the contrary, in stretching out the forepaws, as if imploring the experimenter to release it from the task (the latter, however, is rarer). At the same time, these reflexes are never fully inhibited; they are often disinhibited, i.e., greatly deteriorate in comparison with the degree of inhibition obtained previously. The following phenomenon is usually observed: when we subject the cortical inhibition in such animals to severe strain by means of very delicate differentiation, or by a frequent or protracted application of difficult inhibitors, their nervous system becomes fully, or almost fully, deprived of the inhibitory function; real neuroses set in, typical and chronic nervous diseases, which must be treated either by allowing the animals a very long rest, i.e., by a complete discontinuance of the experiments, or by giving bromide. Together with such animals, there are others in which both nervous processes are at an equally high level.

Consequently, the strong animals are divided into two groups—equilibrated and unequilibrated. Unequilibrated animals belonging to the category described above are met with quite often. It might seem that there should also be unequilibrated dogs of another kind, namely, with a predominance of the inhibitory process over the excitatory. But so far we have not met with such absolutely incontestable cases, or at least we have not been able to discern them.

However, we have had fairly obvious and not infrequent cases when, after a time interval and with the help of gradual and repeated exercises, the initial disequilibrium levelled out to a considerable degree. And this is just another instance when the natural type of nervous system proved to be disguised to a great measure as a result of lifetime training.

Thus, we have a perfect group of strong and equilibrated dogs. However, the animals with this type of nervous system differ greatly, even in appearance. Some are extremely reactive, mobile and lively, i.e., as it were, extremely excitable and alert. Others, on the contrary, are only slightly reactive, sluggish and self-contained, i.e., in general, so to speak, little susceptible to excitation, inert. This difference in the general behaviour must, of course, be due to a specific property of the nervous system and may be best accounted for by the mobility of the nervous processes. Like everybody else we long ago observed this external difference between animals, but we lag considerably in elucidating, on the basis of the conditioned reflex activity, its cause—the mobility of the nervous processes. Only now is this mobility being systematically investigated on two dogs—strongly pronounced representatives of the latter group. Strong and equilibrated, these animals differ greatly in external behaviour. On the one hand, we (Petrova) have an exceedingly lively and reactive animal, on the other (Yakovleva)—an extremely inert and indifferent one. The different mobility of the nervous processes in these animals is distinctly manifested in their conditioned reflex activity which, unfortunately, was not investigated in identical experiments.

The first animal ("Boy") even in the course of usual experimentation with conditioned reflexes displays an amazingly rapid transition from extreme excitation at the beginning—when being placed in the stand and equipped with the apparatus—to a state almost of petrifaction, to a statuesque posture, and, at the same time, to a good working state in the course of the experiment. In the intervals between the

conditioned alimentary stimuli the animal remains in a very strained posture, evincing no reaction to extraneous accidental stimuli; but under the action of conditioned stimuli a strictly recurring salivary reaction sets in immediately, and the dog gulps the food placed before it. Subsequently, this high mobility of the nervous processes, their rapid interchange, manifested themselves, so to speak, with incredible force also in the course of special experiments. In our "Boy" we long ago elaborated two opposite conditioned reflexes to a metronome; one frequency of the metronome acted as a positive conditioned alimentary stimulus, while the other acted as a negative inhibitory one. We then began to reverse the action of the metronome. The negative stimulus was reinforced, i.e., it had to be transformed into a positive stimulus, while the positive one was no longer accompanied by feeding and had to be converted into an inhibitory stimulus. Next day we were able to observe the onset of this reversal and by the fifth day it had been fully accomplished—a rare case of such rapid transformation. One day later an error was made—the metronomes were applied in accordance with their previous significance, namely, the old positive stimulus was again reinforced, while the old inhibitory stimulus was left without reinforcement; as a result, the old relations were immediately reestablished. When the error was corrected, the new relations again quickly reappeared. But this dog presented a truly wonderful, unprecedented example of the formation of a delayed reflex. Generally the elaboration of a delayed reflex, when one and the same stimulus during different periods of its action produces now an inhibitory, now an excitatory effect, is in itself a difficult task. But its elaboration after a long experience of short-delayed reflexes, and even during it, is a truly complicated task, one that cannot be accomplished by the overwhelming majority of dogs and which in successful cases requires much time, even many months. Our dog accomplished this task in the space of few days.

What an extraordinary rapid and free use of the two opposite processes!

All that has been said about this dog entitles us to state that it represents the most perfect type, since it ensures strict equilibration with all that is taking place in the external environment, no matter how strong the stimuli are —both those to which the response must be positive activity, and those the effect of which must be inhibited—and no matter how quickly these different stimuli may interchange. It should be added that these extremely difficult tests were endured by the dog after it had been castrated.

The very opposite, in relation to the property of the nervous system under consideration, is the other dog ("Zolotisty," used by Yakovleva), whose general behaviour has been characterized above. Particularly manifest in the study of the conditioned reflex activity of this dog was the impossibility of obtaining a constant and adequate salivary alimentary reflex; it fluctuated chaotically, often falling to zero. What did this signify? If the reflex tended to be strictly related to the moment of reinforcement, i.e., of feeding, why did it fluctuate and not become constant? This could not have been caused by insufficient inhibition, since we knew that the dog could endure protracted inhibition. Besides, the absence of preliminary salivation is by no means a manifestation of perfection; on the contrary, it indicates an obvious defect. Indeed, the importance of this salivation consists in the fact that the food introduced into the mouth immediately meets with the substance it needs. That this interpretation conforms to reality is proved, in the first place, by its universality, and, in the second place, by the fact that the extent of the preliminary salivation, which is biologically indispensable and important, always strictly corresponds to the amount of food. The natural explanation for the peculiarity of our dog must be sought in the fact that the initial inhibition, which exists in each delayed conditioned reflex—the period of retardation (or the latent period, as we called it previously)—although strong, is obvi-

ously insufficiently labile to keep within the proper time, and owing to inertness, oversteps the normal limits. None of the measures aimed at obtaining a constant salivary effect was successful.

Since the excitatory and inhibitory processes were strong in the dog, it was offered a very difficult task, one, however, that is satisfactorily solved by some other dogs. Among other elaborated conditioned stimuli, and at different moments of this system of reflexes, a new stimulus was applied four times in the course of the experiment, but it was reinforced only when applied the last time; this was a task which required all the resources of the nervous system, and above all a high mobility of the nervous processes. Our dog did its best to solve this problem in a roundabout way, holding on everything which could be a simple, ordinary signal of the fourth reinforced application of the new stimulus. First of all it made use of the noise produced by the food receptacle which was moving before its eyes; during the first three applications of the new stimulus, when no food was offered and consequently no movement of the food receptacle took place, the dog remained in sitting posture. When, during the intervals between the stimulations, empty food receptacles were placed before it in order to deprive it of the signal connected with the reinforcement, it looked into them to see whether there was any food, and only when this was the case, did it stand up (usually it was sitting). When the receptacle was placed too high so that the dog could not see whether it contained anything, it rejected the food altogether, remaining in sitting posture regardless of the stimulus applied. In the case of a positive stimulus, it was necessary to enter the chamber and show the dog that the receptacle contained food, i.e., to invite it to eat, and only then did it begin to eat. Then both the new stimulus and the presentation of empty receptacles were discontinued. Only the old stimuli were applied, of course accompanied by reinforcement. And only *gradually* did the dog begin to rise under the action of the stimuli and to eat.

Again the reflex evoked by the empty receptacle was extinguished. The dog continued to rise under the action of the old conditioned stimuli but—which was the usual thing with it—did not always exhibit any preliminary secretion of saliva. Now the new stimulus was again applied four times, being reinforced only the last time; during the first three applications the food receptacle was not placed before the dog, since, as has just been mentioned, the reflex to it had been extinguished. This time, too, the problem was solved by means of a simple, but new signal, namely, a complex stimulus formed from the new stimulus plus the noise of the moving food receptacle. When the new stimulus was applied for the first three times without the addition of the last stimulation, there was no reaction. But when during these first applications the receptacle was placed before the dog, but with no food in it, i.e., when the complex stimulus was depreciated, the dog, after rising several times in vain, definitely and completely ceased to react to the new stimulus, rising only under the influence of all the other stimuli. Then it was decided to restore the extinguished reflex to the new stimulus, abolishing all other stimuli and reinforcing the new stimulus eight times in succession in the course of the experiment. The rehabilitation of the reflex proceeded *very slowly*. The new stimulus was reinforced in the course of two days, that is, sixteen times, but despite the fact that the experimenter entered the chamber more than once and during the action of the new stimulus showed the food to the dog (only after which it rose to its feet and began to eat) it never stood up by itself under the action of the new stimulus. At first the same thing was observed on the third day; only during the nineteenth application of the new stimulus, when it was prolonged after the expiration of the usual thirty seconds and when new food receptacles were placed at intervals of ten seconds, did the dog, at the fourth presentation of a food receptacle, rise and eat the food. And only later, at first with considerable omissions on the part of the animal, a motor alimentary reflex formed; for the pur-

pose of accelerating its full restoration the dog was more than once left without food for a space of twenty-four hours. Afterwards, on the fifteenth day, there finally developed a full reflex accompanied by a preliminary secretion of saliva, but, inconstant, as usual. On the twentieth day, in order to obtain a constant salivary reflex, the dog was given only half the usual portion of food and this reduced ration was offered for a period of ten days. But the aim was not achieved—the salivary reaction remained inconstant, and even the motor reaction manifested itself either at the end of the action of the conditioned stimulus or only after the presentation of the food receptacle. What striking inertness of the *inhibitory* process! After this, for a period of fourteen days, the dog was given only a quarter of the normal quantity of food, but this, too, hardly changed the picture as far as the reflexes were concerned.

Against this background we began once again to elaborate a new and extremely simplified differentiation: in strict alternation the new stimulus was now reinforced, now not; it was necessary to elaborate reflexes to a single rhythm. In a period of eight days we failed to observe even the slightest trace of a reflex. What striking inertness of the *excitatory* process! Thinking that this phenomenon was partly due to excessive alimentary excitability we increased the quantity of food to half the usual ration. As a result, the difference in the extent of the salivary reaction under reinforced and non-reinforced stimuli now began gradually to manifest itself, and finally a stage was reached when, in the case of reinforced stimuli, the reaction became very considerable, while in the case of non-reinforced stimuli it fell to zero. However, the motor reaction persisted in all cases, although under positive stimuli it appeared quicker. When the experiments were prolonged in order to obtain a complete differentiation also of the motor reaction, the dog began to whine, at first before the experiment and then in the course of it, and tried all the time to escape from the stand. The motor reaction under a non-reinforced stimulus was fully differ-

entiated in some experiments only when it came first in the experiment. The more time passed, the more difficult became the state of the dog; it no longer entered the experimental chamber of its own accord and when taken forcibly would turn back and run away. While in the chamber it kept on howling and barking. Under the action of stimuli the howling and barking became louder. This general behaviour was in striking contrast with the previous behaviour of the animal over a period of three years. In order to help the dog to attain complete differentiation, it was given a full daily ration of food; it gradually calmed down, went to the stand willingly, stopped howling and barking. At the same time a secretion of saliva was observed also under the action of a non-reinforced stimulus; then the salivary secretion induced by the action of the two kinds of stimuli began steadily to diminish until it reached zero. Finally, the motor reaction to a repeated stimulus also fully disappeared. The dog refused to perform its task and lay quietly throughout the experiment, searching for fleas or licking its body. After the experiment it devoured its food with avidity.

Thus, during the long period of the elaboration of a differentiation (the latter being at first difficult, and then quite simple) we observed the extreme inertness both of the excitatory and inhibitory processes. Particularly interesting and clear as to its mechanism was the last period—when a simple differentiation was being elaborated. Owing to a considerably heightened alimentary excitability this differentiation was at last almost completely worked out, but it was accompanied by extreme excitement on the part of the animal; this testified to the difficult state of its nervous system. But when the alimentary excitability declined to the level usually displayed by all the dogs during the experiments, our previous success in keeping the opposite nervous processes within the time limits required by the external conditions was reduced to naught. It proved more difficult for the dog to interchange the excitatory and inhibitory processes at intervals of five minutes, i.e., to maintain the al-

most elaborated procedure, the already formed nervous stereotype, than to repress the rather strong alimentary excitation, under which all our dogs worked quite satisfactorily during the experiments; this excitation was also in evidence in our dog, as proved by the fact that it eagerly devoured the food placed before it after the experiments. This fact strikingly testifies to the great importance of the normal mobility of the nervous processes, as well as to its obvious and considerable insufficiency in our dog, whose nervous processes, however, possessed great strength.

It is now possible clearly to see how the Greek genius, personified (individually or collectively) by Hippocrates, succeeded in discerning the fundamental features in the multitudinous variations of human behaviour. The singling out of melancholics from the mass of people signified the division of the entire mass of human beings in two groups —the strong and the weak, since the complexity of life must, naturally, tell with particular force on individuals with weak nervous processes and darken their existence. Thus, the paramount *principle of strength* was clearly stressed. In the group of strong individuals the choleric is distinguished by his impetuousness, i.e., inability to repress his temper, to keep it within the proper limits; in other words, he is distinguished by a predominance of the excitatory process over the inhibitory. This, consequently, established the *principle of equilibrium* between opposite processes. Finally, by means of a comparison between phlegmatic and sanguine types the principle of the mobility of the nervous processes was established.

There remains the question whether the number of basic variations of human and animal behaviour is confined to the classical figure "four." After years of observations, and as a result of numerous investigations on dogs, we acknowledge at any rate, for the time being, that this number conforms to reality; at the same time we admit that there are minor variations in the basic types of nervous system, especially in the weak type. In the strong unequilibrated type, for ex-

ample, the animals with a particularly weak inhibitory process and, at the same time, quite a strong excitatory process, stand out. In the weak type the variations are, above all, based on the same properties which underlie the subdivision of the strong type into equilibrated and unequilibrated, active and inert animals. But in the weak type the feebleness of the excitatory process, so to speak, depreciates the significance of these other properties and actually makes this type, to a greater or lesser degree, an invalid one.

Now I shall dwell in more detail on the methods, on the more or less definite forms of experimentation already mentioned and which clearly disclose the basic properties of the types; I shall also touch on other, less manifest, forms, which are capable of demonstrating the same properties, though not so distinctly, and at the same time reveal to a greater degree the complexity of the type, even its entire outline. It should be added, however, that many forms of our experiments have not yet assumed definite importance in the solution of the problem of types. Of course, were our knowledge of the subject complete, everything observed by us in our animals, everything recorded by us, would find its proper place in this problem. But this is still far from being the case.

We have already mentioned a definite method of ascertaining the strength of the excitatory process, believing that this strength is most inherent in the strong type. It is a physically most powerful external agent which the animal is able to endure and to turn, along with other less powerful stimuli, into a certain signal, a conditioned stimulus, which remains active for a long period. For this purpose we usually apply very strong sounds, produced by a special rattle which our ear endures with difficulty. In some dogs this stimulus, when reinforced, could be developed, equally with all others, into a real conditioned stimulus, and even take first place among them by virtue of the law of proportionality between the extent of the effect and the intensity of the external stimulus. In other dogs, in accordance with

the law of maximum, its effect declined compared with the other strong conditioned stimuli, however, without interfering with the action of the other stimuli. In still other dogs, when applied, it led to the inhibition of the entire conditioned reflex activity, without becoming a conditioned stimulus. And finally, there were dogs in which one or two applications of this stimulus immediately evoked a chronic nervous disorder—a neurosis which did not disappear of itself and had to be treated.

The second method employed in the case of conditioned alimentary reflexes consists in augmenting alimentary excitability by means of a more or less protracted state of hunger. As a result, in dogs with a strong excitatory process the effects of the strong stimuli, in some cases, are increased; however, there also takes place a relatively greater increase of the effects of weak stimuli, so that they fully or almost fully approximate to the effects of the strong stimuli. In other cases the effects of the strong stimuli remain unchanged, since they have reached their limit and have even somewhat overstepped it; and only the effects of weak stimuli increase, to the degree that they may even exceed the effects of strong stimuli. But in dogs with a weak excitatory process, a heightened alimentary excitability usually leads to a decline in the effects of all stimuli.

The two methods make it possible to determine directly the maximum possible tension of the nerve cell, the limit of working capacity, either directly by the application of extremely strong external stimuli or through the action of stimuli of average strength, provided there is heightened reactivity of the cell, that its state is labile, which is essentially the same thing.

The third method consists in administration of caffeine. In the strong type a definite dose of caffeine increases the effect of the excitatory process; in the weak type it diminishes this effect, causing the cell to overstep the limits of its working capacity.

The weakness of the excitatory process is manifested with particular distinctness, perhaps, in the following experiment; it relates to the course of the excitatory process during the period of the isolated action of the conditioned stimulus; the ascertainment of the effect is facilitated by dividing this period into smaller time units. Three cases are possible: the effect of stimulation may increase regularly and progressively until it is joined by the unconditioned stimulus; it may, on the contrary, be considerable at the beginning and then gradually diminish; and finally, fluctuations of the effect may be observed—now increasing and now declining during the above-indicated period. This fact can be interpreted in the following way. The first case might indicate the presence of a strong excitatory process developing irresistibly under the unceasing action of the external stimulus. The second case, on the contrary, can be interpreted as the manifestation of a weak process for the following reason. In particular cases, for example, after local extirpations of the cerebral cortex, when under usual conditions the effect of the corresponding stimulus disappears, it is still possible to re-establish it in a very weak form in the course of the following experiment. At first the corresponding stimulus is applied several times, being reinforced each time almost immediately after the beginning of its action (in one or two seconds); then, when there is a considerable delay (twenty to thirty seconds), a positive effect is observed immediately after the beginning of the stimulation, which, however, declines rapidly, falling even to zero by the end of the isolated action of the stimulus. This is an obvious manifestation of the weakness of the excitatory process. Finally, the third case which is simply a struggle of opposite processes; the isolated action of the conditioned stimuli leads first to the development of inhibition, since each of our conditioned reflexes is a delayed reflex, i.e., one in which the excitatory process, being premature, must, for a longer or shorter period, be preceded by an inhibitory process and temporarily eliminated.

An absolute, and not relative, determination of the strength of the inhibitory process can be effected, above all, by testing its duration, i.e., by finding out how long the nerve cell can endure a state of continuous inhibition. As mentioned above, the main principle underlying this distinction is as follows. The strong, but unequilibrated, animals, as well as the weak ones, cannot endure a protracted inhibition, with the result that the entire system of conditioned reflexes is temporarily disturbed, or a chronic nervous disorder—neurosis—sets in. The strong animals cannot endure this, since they possess a very strong excitatory process to which the inhibitory process, being sufficient in itself, does not correspond as far as intensity is concerned; this is a case of relative weakness of the inhibitory process. In weak animals both the excitatory and inhibitory processes may be weak—this would be a case of absolute weakness. When the inhibitory process is strong (specially differentiated) its instantaneous or chronic prolongation to a period of five to ten minutes may not evoke any disturbance at all, or cause only a very slight one. But when the inhibitory process is weak, its chronic prolongation, for example, to thirty seconds instead of fifteen, often cannot be effected without causing serious consequences; a prolongation to five minutes, even if effected once, is sufficient to cause a failure of the entire conditioned reflex activity in the form of a persistent neurosis.

The second essential index of the strength of the inhibitory process is its ability rapidly and exactly to concentrate. Usually when an inhibitory process begins to develop at a definite point, it invariably first irradiates and produces a prolonged, successive inhibition. But as soon as the animal possesses a strong inhibition, the latter inevitably begins to concentrate to an ever-increasing degree and, finally, the successive inhibition wholly or almost wholly disappears. When the inhibition is weak, it may remain forever in a more or less pronounced form. The concentration of a strong inhibition entails an acute positive induction, i.e.,

one which appears immediately or after a short period of time; it is manifested in heightened excitability both in relation to the stimulus closest in time, and to the positive stimulus at the point of inhibition (on its termination).

Another index of the strength or weakness of the inhibitory process is the duration of the development of the inhibitory conditioned reflexes; the delay in elaborating an inhibitory reflex may be due to the very great strength of the excitatory process and, consequently, to the relative weakness of the inhibitory process, as well as to an absolute weakness of inhibition. But the end of the elaboration is still more instructive. No matter how long the elaboration of an inhibitory process may last, it remains incomplete forever; more often this takes place when the excitatory process is strong, when there is a relative weakness of the inhibitory process. In some cases the inhibitory process is obviously insufficient and reveals constant fluctuations, even to the extent of complete disappearance; this usually occurs in weak animals with an absolutely weak inhibitory process.

The weakness of the inhibitory process is also expressed in the fact that an almost complete inhibitory conditioned reflex can be obtained only when in the course of the experiment it is evoked first, before any of the positive conditioned reflexes; but if it is evoked in between the latter, it becomes considerably or almost completely disinhibited.

Finally, the absolute weakness of the inhibitory process may also be seen from the animal's attitude towards bromide. In weak dogs only very small daily doses of bromide, not more than a few centigrammes, or even milligrammes, and at most amounting to several decigrammes, prove to be efficient and useful, i.e., maintain a considerable conditioned reflex activity. This fact is explained as follows: since bromide undoubtedly bears a relation to the inhibitory process, in the sense that it strengthens it, only a slight intensification of this process under the influence of

bromide can be endured when there is an inborn weakness of the inhibitory process.

Probably the following phenomenon, too, should be taken into consideration when determining the strength or weakness of the inhibitory process. When a differentiation is elaborated along with a positive stimulus, two contrary consequences are usually observed: either the effect of the positive stimulus increases, or, on the contrary, there is a decline of the effect compared with the level before the differentiation. What do these facts signify with regard to the strength of the nervous processes? It can be assumed that here it is a question of the strength or weakness precisely of the inhibitory process. In the first case a strong inhibitory process concentrates and causes a positive induction; in the second case, being weak, it irradiates and continuously reduces the effect of its positive stimulus. A comparison with other more precise indicators of the strength of the nervous processes may help to establish exactly the mechanism of this phenomenon.

As regards determining the mobility of the nervous processes, until recently we, as mentioned above, did not pay special attention to this particular property of the nervous processes: hence, we do not possess, or to be more exact, have not contemplated any special methods for determining it. Consequently, the job of elaborating them still remains, or the corresponding experimental forms must be selected from among those already at our disposal.

Perhaps a special and most precise method could be elaborated by means of trace conditioned reflexes. By changing, on the one hand, the duration of the indifferent stimulus, which must be turned into a special trace conditioned stimulus, and, on the other hand, the interval between the end of the indifferent agent and the beginning of the unconditioned stimulus that reinforces it, we shall be able directly to measure the degree of inertness or lability of the given nervous system. It can be anticipated, for instance, that, depending on the time needed for the disappearance of the

trace of the stimulus which has ceased to act, the above indicated interval will be of essential importance for a quicker or slower elaboration of a trace conditioned reflex, or even for the possibility of its elaboration in general. The duration of the indifferent stimulus will likewise make itself felt. It is conceivable that in a particularly inert nervous system there will be specially and rapidly revealed for this stimulus the minimum duration under which it is still possible to elaborate a trace reflex.

Next come the methods already tried on two of our dogs which exhibited a striking contrast with regard to the mobility of their nervous processes and which have been cited above as examples. We shall now dwell on them in more detail, partly for the purpose of their further methodical examination and possible perfection, and partly, with the object of elucidating the mechanism of their action.

It might seem that the last method, applied to the inert dog and consisting in a regular rhythmic reinforcement or non-reinforcement of one and the same stimulus, which determined the elaboration of the respectively interchanging excitatory and inhibitory processes, is specially designed to reveal the mobility of these processes. However, this must be proved in a more precise way. By varying systematically in one and the same dog, as well as in dogs belonging to different types of nervous system, the duration of the interval between the reinforced and non-reinforced stimuli and by comparing the results, one can become fully convinced of the essential role played in this respect precisely by the mobility of the nervous processes. This has been just tested on the dog in question. After the summer recess last year the dog finally coped with the required rhythm at usual intervals of five minutes between the stimuli. When the intervals were reduced to three minutes, the rhythm became markedly disturbed. Consequently, the successful elaboration of a rhythm in different animals depends on the intervals, that is, on the degree of mobility of the

nervous processes. The longer the required interval, the lower the mobility, and vice versa.

In order to elucidate the mechanism, I must speak in more detail about the complicated experiment (unsuccessfully performed on the same dog) consisting in an unusual elaboration of a conditioned stimulus from an external agent; this stimulus repeated several times in the course of the experiment among other elaborated conditioned stimuli was reinforced only when applied the fourth time. Successful solution of the problem could be attained subject to complete exclusion of the action of all other reflexes on the repeatedly applied agent. Only on this condition was it possible to establish a differentiation between the first repetitions of the agent and its last application. This probably occurs in the same way as the elaboration of a differentiation between different moments of a protractedly acting stimulus in the case of a considerably delayed conditioned reflex, when to the initial phases of the action of one and the same prolonged stimulus there develops an inhibitory reflex, and to the later phases—a positive reflex. Otherwise, i.e., under the action of other stimuli, the excitatory process evoked by the repeatedly applied agent would not show regular fluctuations depending exclusively on the repetition of the agent, but would fluctuate accidentally and irregularly, depending in each case on the diverse influences of previously applied changing stimuli; hence, no differentiation between different applications of the repeated agent could be elaborated. Consequently, only a high mobility of the nervous processes, i.e., a rapid development and discontinuance of the processes caused by all the other stimuli applied in the experiment, including, of course, the process of eating, could ensure the successful solution of the problem. It should be added that this difficult problem was, nevertheless, solved by another dog, although after a longer period of time and with much greater and more painful strain (experiments of Virzhikovsky). The effect produced by the first three applications of one and the same new

external agent, varying its place in the system of other positive and negative conditioned stimuli, was inhibited; only the last, fourth, application became a constant, durable conditioned stimulus. Since in this dog the conditioned salivary reaction always preceded the addition of the unconditioned stimulus, our inert dog naturally could not make use of any extraneous signals, and consequently, the differentiation between separate applications of one and the same agent could take place only due to the distinction made by the peripheral receptor and the corresponding nerve cell between the last and the first three applications.

Hardly anything can be added to what has already been said about the methods and experimental forms testifying to the lability of the nervous processes in our first dog. The transformation of contrary conditioned stimuli into stimuli of opposite action is obviously determined, above all, by the mobility of the nervous processes which rapidly adapt themselves to the requirements of the new external conditions. This is generally proved by the greater or lesser difficulty with which this procedure is endured even by many strong equilibrated animals, to say nothing of weak and almost all castrated animals, which, as a rule, fall into a chronic morbid state. Similarly the other experimental form applied to this dog, namely, the rapid elaboration of a considerably delayed conditioned reflex among other short-delayed conditioned reflexes applied much earlier, of course, directly testifies to the high mobility of its nervous processes. The new excitatory process, despite the firmly established stereotype in the action of other stimuli, rapidly adapted itself to the requirements of the new condition, at first being replaced by a durable inhibitory process and then just as quickly reappearing after slight modification in the course of its development, a modification which more closely coincided with the application of the unconditioned stimulus.

Experiments with a direct transition from an inhibitory to an excitatory process and vice versa should likewise be included in the category of experimental forms ascertain-

ing the mobility of the nervous processes. We know that in certain dogs this transition is accomplished easily and with exactitude. Sometimes, in particularly perfect types, the direct precedence of the inhibitory process, owing to its positive induction, determines even an increased effect of the positive stimulus; but in weak types this is usually accompanied by a breakdown, i.e., by a more or less serious nervous disorder.

The so-called reshaping of the stereotype,[32] that is, a certain change in the sequence of a repeatedly applied system of the same conditioned reflexes (for example, a fully inverted sequence) must also be related to this category of experimental forms. In some dogs this change does not exert even the slightest influence on the effects of the different stimuli; in others it is sometimes accompanied by complete disappearance of the conditioned salivary reaction for days (in the case of alimentary conditioned reflexes).

In old age it often happens that the systems of conditioned reflexes, previously reproduced in a regular and stereotype way, i.e., with precise effects of the stimuli, become irregular and chaotic; the precision and constancy of the effect can be re-established only by a simplification of the system—either by the exclusion of negative reflexes, or by a simultaneous reduction of the number of positive reflexes. It would be most natural to explain the mechanism of these facts by a decline, above all, in the mobility of the nervous processes, brought about by old age, as a result of which the inertness and duration of the processes, at previously established intervals, lead to a confusion and collision of the effects produced by the different stimuli.

Certain morbid disturbances observed in our dogs when they have to solve difficult nervous tasks, expressed in pathological states of definite cortical points, should be also ascribed to pathological changes in the mobility of the nervous processes; such are the inertness and explosiveness of the excitatory process. On the one hand, it was frequently observed that the excitatory process of an iso-

lated point of the cortex became abnormally tenacious: the effect of the conditioned stimulus connected with it was not susceptible to inhibition by preceding inhibitory reflexes to such a degree as the effects of other stimuli; its extinction proceeded much more leisurely, and this stimulus did not lose its positive action, in spite of the fact that it was not reinforced systematically for weeks and months (Filaretov, Petrova). On the other hand, the previous stimulus, which had acted normally and whose moderate effect appeared after a certain delay, increasing after the addition of natural alimentary stimuli and ending in the normal act of eating upon presentation of food, now, under a pathological state of the corresponding point of the cortex, began to produce a tremendous (secretory and motor) effect, arising and ending abruptly. When food was offered, the dog violently and obstinately rejected it (experiments of Petrova). It is clear that an extreme lability of the excitatory process was in evidence and that the latter, especially due to its summation with natural alimentary stimuli, rapidly reached the limit of working capacity of the cortical cell and evoked a very strong transmarginal inhibition.

Thus, I repeat, the possible variations of the basic properties of the nervous system, as well as the possible combinations of these variations, determine the types of nervous system; as calculated, their number amounts at least to twenty-four. But life shows that the actual number is considerably smaller: we distinguish four types which are particularly distinct and strongly pronounced, and, what is most important, differ in their adaptability to the external environment and their resistibility to morbific agents.

We must admit a type of *weak* animals, characterized by a manifest weakness both of the excitatory and inhibitory processes; they never fully adapt themselves to the conditions of life, are easily broken, often and quickly become ill and neurotic as a result of difficult life situations, or, what is the same thing, of the difficult nervous tasks which

we place before them. But of still greater importance is the fact that this type, as a rule, cannot be improved to any considerable degree by training and discipline; it becomes fit only under particularly favourable, deliberately created, conditions, or, as we usually say, in hot-house conditions.

The type is in contrast to the types of *strong* animals which in their turn markedly differ.

Among the latter, in the first place, is the *strong*, but *unequilibrated* type with a strong excitatory process, but with a weaker, and sometimes even a considerably weaker, inhibitory process, in view of which this type is also easily subject to pathological disturbances when inhibition is required. This, predominantly, is a fighting type, but not adapted to everyday life with all its fortuities and exigencies. Nevertheless, being strong, it is capable of disciplining itself to a considerable degree, improving thereby the originally insufficient inhibition. We term it the *excitable type*, but to avoid misunderstanding and confusion it would be better to use the adjective *impetuous*, which directly stresses its defect and at the same time obliges us to regard it as a strong type.

From this strong type one must single out the *strong* and *equilibrated* animals.

But these animals, in their turn, differ greatly, first of all in external behaviour, and this, as we already know, is precisely due to the mobility of the nervous processes. In order to designate these *strong* and *equilibrated* types we can correctly accord them the attributes *calm* and *lively*, in conformity with their mobility.

Such are the principal types which exactly correspond to the ancient classification of the so-called human temperaments—melancholic, choleric, phlegmatic and sanguine.

As for the less significant variations, they are most frequently met with, as already mentioned, in the weak type, but they have not yet been fully investigated and systematized by us.

In conclusion I wish to say a few words about the frequency of these types among the multitude of dogs of various breeds that have passed through our laboratories during our study of the conditioned reflexes. The *weak* type in all its variations and the *lively,* sanguine type are the most frequent; then comes the *impetuous*, choleric type; rarest is the *calm*, phlegmatic type.

Basing ourselves on the elementary physiological principles underlying the classification of the types of nervous system in animals, we must admit the same types in the mass of human beings—a classification already made by Greek classical thought. Thus, Kretschmer's[83] classification of nervous types, which has obtained almost universal recognition, especially among psychiatrists, must be regarded as mistaken or inadequate. Kretschmer found his types in the clinic, among the ill. But are there not absolutely healthy individuals? And why must all human beings indispensably carry nervous and mental disorders in embryo?

Kretschmer's types represent only a part of all human types. His cyclothymics[84] are closest to our excitable, impetuous type, or to Hippocrates' cholerics, and his schizothymics[85]—to our weak type, or to Hippocrates' melancholics.

Since the first type lacks a proper abating and restorative process—the process of inhibition—its excitatory process often considerably exceeds the working capacity of the cortical cells. This causes a derangement of the proper interchange of normal work and rest, which manifests itself in extreme morbid phases of the excitatory and inhibitory states, both with regard to intensity and duration. Hence, the eventual development of a manic-depressive psychoses[86] under particularly difficult circumstances of life, or under certain unfavourable conditions of the organism.

In the second type both processes are weak, and because of this it cannot endure individual and social life with its

severe crises, which mostly fall on a still young, not sufficiently adjusted and hardened organism. This may lead, and often does lead, to a complete destruction of the higher part of the central nervous system, unless some lucky chance in life, or, more often, the protective function of the inhibitory process, does not save it from disastrous overstrain during this difficult period. It can be rightfully assumed that for those representatives of the weak type who end up with schizophrenia[87] there are certain specific conditions, such as a particularly irregular course of development, or permanent auto-intoxication, causing extreme fragility of the nervous apparatus. Aloofness or reticence which, according to Kretschmer, is the main feature of schizothymics from childhood, does not present anything specific; in the case of a weak nervous system it is merely a general indication of the extreme complexity of the social environment; hence the natural withdrawal from it. Is it not a widely recognized and current fact that the mere transfer of a nervous person to a clinic or sanatorium, that is, the simple act of removing the patient from his everyday surroundings, affords relief and is even of curative importance?

It should be added that reticence or aloofness from society is by no means an exceptional feature of schizothymics, i.e., of weak individuals. Even strong persons may be reserved, but for quite different reasons. This type of person leads a strenuous but at the same time one-sided subjective life; he early becomes possessed by a certain inclination, concentrates on a single aim and is dominated and carried away by a single idea. Other people are not only undesirable; they even disturb him and distract him from the principal object of life.

Naturally, there are many great men also among cyclothymics (the strong type). But it is understandable, that, being unequilibrated, they possess a particularly fragile nervous system. Hence, the widespread and vividly discussed problem: genius or insanity?

And then comes, of course, the multitude of human beings more or less strong and even exceedingly so, and at the same time equilibrated, the phlegmatics and the sanguines, the people who make the history of mankind either by their systematic mundane but indispensable labour in all branches of life, or by the exploits of their mind, lofty emotions and iron will. Of course, as far as great men are concerned, no matter how strong they may be, they are also subject to breakdowns, since the scale of their activity is extraordinary, and there is a limit to any strength.

VIII

PROBLEMS OF SLEEP AND HYPNOSIS

SOME FACTS ABOUT THE PHYSIOLOGY OF SLEEP[88]

(JOINTLY WITH DR. L. N. VOSKRESENSKY)

In our study of the so-called conditioned reflexes we often had to deal with the phenomena of sleep. Since these phenomena greatly complicated our experiments, disturbing them and deflecting them from their normal course, we were, naturally, compelled to devote special attention to them. In addition to accumulating isolated facts, two of our colleagues—N. A. Rozhansky and M. K. Petrova—elaborated this problem most systematically. N. A. Rozhansky investigated that form of sleep, or somnolent state which apparently results from the influence of monotonous, indifferent stimuli, for example, the isolated environment in which the experimental animal is placed. When the animal is enclosed in an isolated chamber and placed in the stand, it gradually becomes drowsy, and then goes into deep sleep. Sleep also occurs under the influence of definite, active stimuli from which strong conditioned stimuli have been elaborated. Under the influence of these stimuli there arises a sleeping hypnotic state in all dogs, and in some of them with particular ease. Recently Dr. L. N. Voskresensky had a case of this somnolent state which for us was somewhat unexpected, since numerous experiments had already been performed on this dog by Dr. A. M. Pavlova and no marked signs of sleep had been observed during those experiments. But now in the course of our research, sleep unexpectedly intervened and constantly disturbed our

experiments with conditioned reflexes; as a result, the usual phenomena were sometimes entirely absent and sometimes distorted. How did this come about? At first we were not quite sure whether this state was really sleep, and attributed the disturbances to other causes. But thorough observation and various tests excluded all other suppositions. All that remained was to admit the development of a state of sleep in the dog. But what caused it? When we closely considered all the details of the recent experiments performed on the dog, it appeared that the sleep was due to the following reasons. Prior to this peculiar period the experiment was usually begun the moment the dog was placed in the stand—it was subjected to the action of special conditioned stimuli and food was given as an unconditioned stimulus. These conditions did not produce a sleeping state. Now, however, due to certain circumstances, the dog was left in the stand for a relatively long time, waiting for the beginning of the experiment. And it was the continuously acting, monotonous surroundings which caused the gradual onset of a state of sleep. This interpretation proved to be perfectly reasonable. As the particulars relating to the development of a state of sleep were of great interest to us, we decided to investigate the question with the utmost thoroughness.

First of all it appeared that from the quantitative point of view the environment acts with surprising precision, i.e., if immediately after the necessary preparations (fixing the different funnels, fastening the apparatus, etc.) you begin the experiment, the usual stimulations of the animal, there are no signs of the phenomena of sleep at all. But should a minute pass between the completion of the preparations and the beginning of the stimulation the first phase of sleep becomes manifest. If ten minutes pass you observe the next stage of sleep and so forth. Thus the sleep-producing influence of the environment can be truly dosed. This made possible an easy study of the course of sleep, of the somnolent state which develops under these conditions.

And here are the results of our observations. During the experiments we usually had before our eyes two reactions of the animal; on the one hand, there was a secretory reaction, a flow of saliva; on the other—a motor reaction—the dog seized the food offered to it. In other words, these were the motor and secretory reflexes. It turned out that the strictly law-governed development of the observed phenomena depends on the quantitative influence of the soporific environment; this is shown in the following table:

State of the dog	Phases of sleep	Reflexes*		Remarks
		Secretory	Motor	
Awake		+	+	
	I	—	+	
Asleep	II	+	—	
	III	—	—	Deep sleep
	II	+	—	
	I	—	+	
Awake		+	+	

In the wakeful state both the secretory and the motor reflexes are present. Immediately after the conditioned stimulus begins to act a secretion of saliva appears, and the dog takes the food as soon as it is offered. Thus both reflexes are effective. Now we keep the dog under the influence of the surroundings at least for two minutes, i.e., when the preparations for the experiment are finished, we let two minutes pass, and then apply the conditioned stimulus. The *first phase of the state of sleep* is then observed. It is manifested thus: the secretory reflex disappears; the conditioned stimulus no longer acts; but when the dog is offered food it immediately seizes it, which shows that the motor reflex persists. Now you augment the influence of the surroundings, i.e., for example, you keep the dog waiting ten min-

* The sign + signifies the presence and the sign — the absence of a reflex.

utes before the experiment begins. Then its sleep deepens, and another kind of reaction is observed, which, strange as it may seem, is of a reverse nature and represents the *second phase of the state of sleep*; the dog exhibits a secretion of saliva, but does not take the food, and even turns away from it. Thus the salivary reaction which is absent during the first phase of sleep, reappears in the second, while the motor reaction disappears or even passes into a negative reaction; the dog not only refuses food but even turns away from it. If the dog is left in the soporific surroundings for a period lasting from half an hour to one hour before the beginning of the experiment, a *complete, deep sleep* sets in, and both reflexes vanish. Now let us wake the dog from its deep sleep. This can be done at once, and the simplest method is to apply a strong sound stimulus. In our laboratory we use a very loud rattle, which wakens the dog instantaneously. The animal immediately returns to a normal alert state. However, a more delicate stimulus may be used.

One of the customary methods of gradually dispelling sleep is to feed the dog at definite intervals; feeding may be even begun with the forcible introduction of food into the mouth. Then you can observe the phases described above but in reverse order. After the deep sleep the secretory reflex is present but the dog does not take the food. Later on, the secretory reflex fails to appear; however, the dog eats. And finally, after frequent repetitions of the feeding, both reflexes reappear. Now I shall call your attention to some authentic figures. Take, for example, a dog that has just been fastened in the stand; we begin to apply certain conditioned stimuli and a secretion of saliva appears. Our scale shows 37 divisions, which indicates a normal salivary reaction. It should be added that the following precaution was observed by us in order to ensure strict precision in our investigation. The chamber itself had a hypnotizing effect on the dog; the moment the lively, mobile and responsive animal was brought into the experimental room,

it changed entirely. It goes without saying that the state of sleep deepened when the dog was placed in the stand and prepared for the experiment. In order to determine exactly the moment of the passage from the wakeful to the sleeping state, we did everything to prevent sleep while the dog was being fastened and the apparatus attached to it; we called it by name, stroked it and patted it. When everything was ready, we would quickly leave the chamber and begin the experiment immediately. In this way we obtained the above-mentioned normal secretory reaction, equalling thirty-seven divisions of our scale; the motor reflex was also present. In the next experiment we allowed the surroundings to act on the animal for a space of two minutes, and the following was observed: the secretory reflex failed to appear at all, not a drop of saliva was secreted in response to our conditioned stimulus, but the dog took the food at once. Next time we let the surroundings act for four minutes; we obtained twenty divisions of saliva but the dog took the food only in forty-five seconds, and even then only when the food was brought into contact with its mouth. Finally, when we allowed the surroundings to act for half an hour to an hour, all the reflexes disappeared.

The procedures were, of course, varied by us, so that we were able to obtain both phases of sleep in one and the same experiment. For example, the dog remained in the chamber for seventy-five seconds; as a result, the secretory reflex was zero, but the food was taken at once. Then we let an hour pass, leaving the dog alone. The excitation produced by a single meal neutralized to some extent the soporific influence of the surroundings, and only the second phase was observed: the secretion of saliva equalled twenty-two divisions, and the dog ate the food only for about half a minute after the food had been brought into contact with the mouth. Here is another concrete instance of how sleep is dispelled. The dog is in a deep sleep and in order to arouse it we apply, among others, a weak stimulus: someone enters the chamber where the dog is

kept in the stand. The noise produced by the person entering the chamber, and perhaps his odour, slightly disturb the animal's sleeping state. If we now apply the conditioned stimulus we obtain twenty-four divisions of saliva, but the dog takes the food fifty seconds later, not of its own accord, however, but only when it is put into its mouth. We then feed the dog once or twice, thereby stimulating it; we dispel the state of sleep and observe a transition to the following phase: the secretory effect is diminished, there are only ten divisions of saliva and the food is taken by the dog after twenty seconds. Whereas in the preceding case the dog ate the food after fifty seconds and from the experimenter's hand, now it takes the food of its own accord and only after twenty seconds. With a fresh stimulus applied after twenty minutes, the secretory reflex is zero, and the dog eats the food almost immediately. Finally, when the next conditioned stimulus is applied, thirty-five divisions of saliva are secreted, and the dog takes the food right away. This signifies the presence of a perfectly alert state. Hence, we must recognize as a thoroughly established fact that the processes of falling asleep and of awakening influence our two reflexes in a strictly definite way. We witnessed a very interesting fact which is, above all, of practical importance, since it enabled us to control the animal and to remove the influences which interfered with the experiment. It sufficed to feed the dog two or three times, or to prevent the surroundings from acting on it at the beginning, to make us masters of the situation; sleep did not disturb our experiments with the conditioned reflexes. Now the question arose: How to interpret this phenomenon? Certainly it is a complicated question and for the time being only an approximate answer can be given. Our colleagues N. A. Rozhansky and M. K. Petrova, on the basis of their experimental data, have come to the conclusion that both states of sleep observed by them represent an inhibitory process and that in one case it spread from several points of the cerebral hemispheres (case of Rozhansky), and in

another case—from one definite point (Petrova). Our fact, it would seem, confirms this conclusion, since in our experiments there was actually in evidence a localization and even a movement of the somnolent state in the cerebral hemispheres. How can this movement of sleep inhibition be better traced in the cerebrum? A similar question arose and was successfully investigated in connection with another kind of inhibition, the so-called internal inhibition. A few months ago one of us addressed you here on this subject. This investigation gives us reason to hope that we may achieve the same with regard to sleep inhibition. The simplest way would be to trace the movement of this sleep inhibition in a definite part of the cerebral hemispheres, since, as shown by our experiments concerning the irradiation of, say, internal inhibition *over the entire hemisphere*, certain circumstances greatly complicating the picture are met with in this case (probably, the border-line layers between different parts of the hemispheres, various degrees of energy of stimulation, etc.). Experiments in this direction are now performed in our laboratory. It is more convenient to trace the movement of sleep inhibition in that part of the cerebral hemispheres which relates to the skin, being, as it were, its projection in the brain. Moreover, the conditioned stimulation of the skin provokes a sleeping state quite easily. If we assume that this sleeping state arises precisely at the point of stimulation, we see how this inhibitory movement spreads from this point over the entire cutaneous area of the brain; it will then be possible to determine how far and how quickly this process spreads. But, for the time being, this is only a hope.

CONCERNING THE SO-CALLED HYPNOTISM IN ANIMALS[89]

The so-called hypnotism of animals (the experimentum mirabile of Kircher[90]) consists in the fact that by means of energetic action, overcoming all resistance, the animal is brought to an unnatural posture (laid on its back) and kept thus for a brief space of time. Afterwards, when the hands are removed from the animal, the latter remains motionless for many minutes and even hours. Different authors, noting one or other detail of this phenomenon, have explained it in various ways. At present, thanks to the systematic study of the normal activity of the brain, I am in a position to indicate the biological significance of this phenomenon and to give an exact and full explanation of its physiological mechanism; thus I am able to combine all the separate facts of the different authors. The phenomenon represents a self-protecting reflex of an inhibitory character. Faced with an overwhelming power, from which there is no escape in struggle or in flight, the animal's only chance of salvation is to remain immobile in order not to be noticed, since moving objects attract particular attention, or not to provoke by fussy, restless movements an aggressive reaction on the part of this overwhelming force. Immobility is brought about in the following manner. Extraordinary external stimuli highly intense or very unusual in form, first of all cause a rapid reflex inhibition of the motor region of the cerebral cortex which controls the so-called voluntary movements. Depending on the intensity and duration of the stim-

ulus, this inhibition is either confined to the motor region and does not pass to other regions of the cerebral hemispheres and to the mid-brain, or it irradiates over all these parts. In the first case there are present reflexes of the eye muscles (the animal follows the experimenter with its eyes), of the glands (when food is offered, there begins a secretion of saliva, although no skeletal movements in the direction of food are observed), and finally tonic reflexes from the mid-brain to the skeletal muscles in order to retain the position into which the animal has been brought (catalepsy). In the second case all the above-mentioned reflexes gradually disappear, and the animal passes into an absolutely passive, sleeping state accompanied by a general relaxation of the musculature. This course of the phenomena is further confirmation of the conclusion which I reached at a previous meeting of our section, namely, that the so-called inhibition is nothing more than sleep, but partial and localized. It is clear that the rigidity and stupor which seize us in cases of great fear is nothing else but the above-described reflex.

P.S. It should be added that during the period when I did not have physiological literature at hand, which I managed to get only in the spring of 1922 in Helsingfors, a number of other authors came to this same conclusion concerning hypnosis in animals.

PHYSIOLOGY OF THE HYPNOTIC STATE OF THE DOG[91]

(JOINTLY WITH DR. M. K. PETROVA)

Besides the usual classical method of hypnotizing animals (laying the animal on its back and keeping it for some time in this unnatural position), which results in a hypnotic state manifesting itself in catalepsy,[92] our laboratories were able, in the course of their research into the normal activity of the higher parts of the brain, to study in more detail the diverse and very delicate manifestations of the hypnotic state. As already established by us, the basic condition required for the development of this state is a prolonged action of monotonous stimuli, which finally bring the corresponding cortical cells to a state of inhibition. This inhibition, on the one hand, is of different degrees of intensity, and on the other hand, spreads to a greater or lesser extent over the cerebral cortex and further down the brain. Corresponding facts were cited in a book published by one of us (I. P. Pavlov, *Lectures on the Work of the Cerebral Hemispheres*).

But subsequent observations revealed a greater variety of symptoms of the hypnotic state, its more and more delicate gradations, which hardly differ from the wakeful state, and its ever-increasing mobility depending on the slightest changes in the surroundings, on insignificant modifications in the external stimuli acting upon the animal.

In the present article we shall deal with the phenomena observed by us in two dogs. Previously they were used by

one of us (M. K. Petrova) for studying the various conditioned reflexes, but now they constantly fall into a hypnotic state the moment they are placed in our usual experimental conditions and respectively equipped.

Long ago in the works which originated in our laboratories it was repeatedly pointed out that in the case of conditioned alimentary reflexes there takes place a dissociation of the salivary secretion and the alimentary motor reaction when the dog falls into a sleeping state. It usually happened that our artificial conditioned stimuli or more often the natural stimulation (which, as has been proved, is also conditioned) produced by the sight and odour of food, evoked a profuse secretion of saliva, although the animal did not take the food. It was in this state of the animal that very diverse and highly interesting variations of the alimentary motor reaction were manifested in the course of our observations. These variations, which apparently represent different degrees of intensity of hypnosis, were now predominantly observed in one animal, now in another. One of the dogs, which was usually in a less profound hypnotic state, distinctly exhibited what in mental diseases is called negativism.[93] After a conditioned stimulation applied during a certain period of time we put food before the dog; the latter turns away from the food receptacle. But when we begin to move the receptacle away, the dog makes a movement in its direction. We present the receptacle anew; the dog again turns away from it. We move it away, and the dog turns towards it once more. We have termed the reaction of turning away from the food receptacle negative, or the first phase of negativism, and the movement towards the food receptacle—positive, or the second phase. This negativism may recur many times until the animal at last partakes of the food, which happens in most cases. The degree of hypnosis is expressed precisely by the number of repetitions of this procedure. At the beginning of the hypnotic state the food is taken and eaten by the dog after the second offering. When the hypnotic state becomes more profound,

both phases of negativism recur more and more often. When hypnosis reaches the highest degree, the dog rejects the food, no matter how many times it is offered. But as soon as the hypnotic state is dissipated in this or that way—for example, by removing the apparatus attached to the dog for the purpose of collecting the saliva, or by loosening the chain which is attached to the dog and which during the experiment is fastened to the upper cross-bar of the stand, or by some other means—the dog immediately begins to devour the food.

In the other dog the alimentary motor reaction during hypnosis assumed an even more complicated form. In one of the more pronounced cases the phenomena developed in the following sequence. Under the action of our conditioned stimuli (usually to the end of their isolated action) the dog, if it was in a sitting posture, rose to its feet, if standing, it turned its body in the direction whence the food was usually presented to it. But when food was offered, it turned its head away from it, thus exhibiting the first phase of negativism. Then the food receptacle was moved away, and the animal, on the contrary, turned the head towards it and followed it with its eyes; thus the second phase appeared. After some manifestations of this negativism the dog at last brought its mouth close to the food, but was unable to take it. As if with great difficulty it began, little by little and repeatedly, to open and to close its mouth, but to no purpose—it did not take the food (abortive movements). Afterwards, it began to move its jaws with greater ease, took the food, at first in small portions, finally opening its mouth wide and swallowing rapidly without interruption. Thus, in this hypnotic phase we must distinguish three different states in three parts of the skeletal musculature relating to the process of eating: strong inhibition, immobility of the muscles directly participating in the process of eating (the masticatory and lingual muscles); considerable mobility, but of a periodic character, in the form of a negativism of the cervical musculature; and finally, normal activity of the remaining mus-

culature of the body. The more profound the hypnotic state, the more immobile and inhibited is the direct musculature: the tongue is put out as if paralyzed, the jaws are absolutely motionless. In the cervical muscles only the first phase of negativism is manifested. Then the movements of the head cease completely, and only the trunk still turns under the influence of conditioned stimuli. Finally, when the hypnotic state becomes still more profound, this last motor reaction to the conditioned stimuli, as well as to food, also disappears. All these phenomena can be dissipated, abolished instantaneously by the same methods which have been described in the case of the first dog.

Concerning the alimentary motor reaction in our cases, the following should be added. Any slight change in the usual appearance of the food, and even in the manner of presentation leads to conversion of the negative motor reaction into a positive one; in other words, the dog eats the food which it has just rejected. For example, we offer the dog in an ordinary cup slightly damped and evenly spread powder of dried meat and bread. The dog refuses it. But if the same powder is presented partly in the form of a lump protruding from the cup, the dog seizes it greedily and then begins to eat the rest of the powder. A positive reaction can be also obtained if the powder is offered to the dog on a small plate or on a piece of paper. It takes the food also from the experimenter's hand instead of from the cup. Finally, sometimes, after the conditioned stimulation, it begins to lick up the powder spilled on the floor, although when offered in the cup, it refused it.

Along with these motor phenomena relating to the process of eating there were manifested in the course of our observations on the hypnotic state other specific motor reactions worth noting. Many dogs, after partaking of their small portion of food and being in an alert state, for a time lick their forepaws and breast. In the hypnotic state the licking usually assumes a protracted character; in the case of one of our dogs in question it soon passes into a peculiar

form. The dog licks the forepaw and wets it with saliva, especially the flesh of the toes; then it brings the forepaw close to the apparatus attached to the salivary fistula and passes the toes over it—a gesture it repeats many times if not stopped. In the alert state the same dog did not do this. Some dogs in the alert state struggle against the apparatus only when it is first attached to them, afterwards they get used and pay no attention to it. We can rightfully suppose that our dog exhibited in the hypnotic state one of the specific defensive reflexes. When a dog has a wound on a part of the skin within reach of its tongue, it repeatedly cleans it with saliva, or, as we say, licks it (the auto-curative reflex). Apparently in this particular case the irritation evoked by the hardened cement, by means of which the apparatus is attached to the skin, is responsible for the manifestation of the reflex; and since the point of irritation is not accessible to the tongue, the latter is replaced by the toes of the forepaw.

Many of the above-described variations of the alimentary motor reaction usually take place during one and the same experiment and rapidly supersede one another. This variability, this mobility of the hypnotic state is also seen in other phenomena. We shall cite a few more cases illustrating the fluctuation of the hypnotic state and the modification of the effect of the conditioned stimulus, already described and reproduced by us, or noted for the first time in the course of our observations and experiments on dogs. These fluctuations and modifications are either due to causes still unknown or are related to definite conditions.

I repeat that, if the dog is susceptible to hypnotization under experimental conditions, the hypnotic state usually develops immediately after the dog is placed in the stand, and sometimes the very moment it crosses the threshold of the experimental chamber. With the progress of the experiment this state grows continuously and gradually, provided it is not dissipated by certain new conditions.

Let us consider first of all the dissociation of the secretory and motor reactions of the alimentary reflex. This dissociation often assumes the form of, so to speak, reciprocal antagonism. In some cases the stimulation evokes a secretion of saliva in the absence of any motor reaction, i.e., the dog, as mentioned above, does not take the food. In other cases, on the contrary, the dog rapidly seizes the food and eats it with avidity, but there is no salivary secretion in response to well-elaborated conditioned stimuli.

Here is the example of one of our dogs—"Bek." The experiment took the following course during two days in succession:

April 17, 1930

Conditioned stimulus	Secretion of saliva in drops during 30 secs.	Alimentary motor reaction
Rattle-box	15	Negativism, then takes food
Bell	15	Abortive movements; rejects food for a long time

April 18, 1930

Conditioned stimulus	Secretion of saliva in drops during 30 secs.	Alimentary motor reaction
Rattle-box	1	Takes food at once, but eats inertly
Bell	0	Takes food at once and eats with relish

Sometimes these, as it were, antagonistic relations between the secretory and motor alimentary reactions rapidly interchange in the course of the experiment.

This can be illustrated by an experiment performed on another dog "John":

April 12, 1930

Beginning of the Experiment

Conditioned stimulus	Secretion of saliva in drops during 30 secs.	Alimentary motor reaction
Rattle-box	5	Negativism
Bell	0	Takes food at once

In the early works that originated in our laboratories it was frequently stated that a well-elaborated inhibitory stimulus, usually a differential one, can modify the hypnotic state in two opposite directions—either intensifying, or weakening it. The same thing was often observed by us in the above-mentioned animals in their state of hypnosis.

Finally, it should be pointed out that among our usual conditioned strong stimuli a particularly powerful conditioned stimulus often eliminates or weakens the hypnotic state whereas stimuli of usual strength either leave it unchanged or even reinforce it.

Here is an example connected with the experiment performed on the above-mentioned "Bek," the beginning of which has been described above. When the experiment was continued and a differentiation applied, the conditioned stimuli of medium strength—the rattle-box, the gurgle of water and the bell—did not produce any secretory effect, and the dog, while making abortive masticatory movements, did not take food for a long time. A strong crackling sound which is a very powerful conditioned stimulus, evoked a

secretion of saliva, and after a short period of negativism the dog took the food.

April 17, 1930

Conditioned stimulus	Secretion of saliva in drops	Alimentary motor reaction
Rattle-box	0	Does not take food for a long time
Gurgling sound	0	Same
Strong crackling sound	5	Negativism of short duration
Bell	0	Does not take food for a long time

How should the physiological mechanism of the above-mentioned phenomena be interpreted and understood? It is evident that at the present level of our knowledge in the field of the physiology of the higher parts of the brain it would be an extreme pretension, incompatible with the real state of affairs, to try to give a well-grounded and clear answer to all questions which may arise in this connection. However, we must constantly attempt to explain particular phenomena by the more general properties of the activity of the higher parts of the brain, to effect new variations of experiments that would ensure a closer approach to the comprehension of the extremely complex relations of reality which exist in the given case.

The difficulty which we meet when attempting to elucidate the mechanism of the above-mentioned phenomena observed in the hypnotic state, is that under stimulations, undoubtedly reaching the cerebral cells, we often do not know what in the ensuing nervous activity should be attributed to the cerebral hemispheres and what to the lower levels, the lower parts of the brain and even the spinal cord. In the course of the philogenic development of the central

nervous system the nervous combinative systems, in the form of definite, so-called reflex centres, becoming more and more complex, steadily moved closer to the brain end; they effected an increasing analysis and synthesis of the stimulating agents due to the augmenting complexity of the organism and the growth of its relations with the external environment in ever-widening areas. Thus, along with a more or less stereotype nervous activity, and with ready complexes of physiological functions, called forth by a limited number of elementary stimulations, there gradually developed the higher nervous activity dealing with an ever-increasing number of conditions, of complex, and besides, variable, stimulations. Then a very complicated problem arises before the investigator, the problem of the connection and of the forms of this connection between different levels of the nervous system. As to our first problem concerning the dissociation of the secretory and motor reactions of our conditioned alimentary reflex, it is necessary to establish what in this reflex should be ascribed to the cortex and what to the adjacent subcortex, or, in ordinary terminology, what in this process is of a voluntary and what is of a reflex character. To be still more exact, it is necessary to know whether in the conditioned alimentary reflex the secretory and motor components equally depend on the cortex, or whether there is a difference between them in this respect. Does not the motor component predominantly depend on the cortex, and the secretory component on the subcortex?

Let us turn to the well-known facts.

Proceeding from the phenomena of human hypnosis we must admit that in the cerebral cortex along with a grandiose representation of the external world effected through the afferent fibres (an indispensable condition for the highest regulation of functions) there is also a vast representation of the organism's internal world, i.e., of the states and functioning of numerous organs, tissues and internal organic processes. In this respect particularly convincing are the facts pertaining to the so-called imaginary, self-sug-

gested pregnancy. Numerous processes relating to the activity of passive tissues, such as the adipose one, arise and become intensified under the influence of the cerebral hemispheres. But it is clear that these two kinds of representation differ greatly in degree. Whereas the representation of the skeletal musculature is highly delicate and detailed, perhaps being equal in these respects to the representation of such external energies as sound and light, the representation of other internal processes lags considerably.[94] This is probably due to the slight practical significance of the representation. In any case, it is a constant physiological fact. And this, apparently, makes it possible to distinguish between the voluntary and involuntary functions of the organism, the former including only the activity of the skeletal musculature. This voluntariness signifies that the work of the skeletal musculature is, above all, determined by its cortical representation, by the motor region of the cortex (the motor analyser, in our terminology) which is directly connected with all the external analysers; in other words, in its orientations it is always determined by the analytical and synthetical work of these analysers.

Proceeding from these facts, we can present the mechanism responsible for the elaboration of our conditional alimentary reflex in the following way. On the one hand, this is a union between the cortical points of application of the conditioned stimuli and the reflex alimentary centre of the adjacent subcortex with all its particular functions; on the other hand, it is a closer connection of the same points with the corresponding parts of the motor analyser, i.e., those which participate in the process of eating. Then the dissociation of the secretory and motor components of the alimentary process taking place in the course of hypnotization might be interpreted as follows. The hypnotization evokes a state of the cortex when the motor analyser is inhibited, while all the other analysers are free. The latter evoke a reflex on the alimentary centre of the subcortex with all its functions, while the inhibition of the motor analyser, so to

say, by direct communication, excludes the motor component from this reflex, thereby bringing the terminal points of movement, the cells of the anterior horns,[95] to a state of inactivity. Thus, in the alimentary process only the secretory reaction remains manifest.

Here is the reverse case. An artificial conditioned stimulus does not produce a secretion of saliva, but a motor reaction is in evidence—the dog takes the food at once. Now this can be easily explained. This must be a weak inhibition of the entire cortex, and an artificial stimulation alone is not sufficient to dissipate it; only with the presentation of food, when the artificial conditioned stimulus is supplemented by natural stimuli (the sight and odour of food, which in themselves are even stronger than artificial stimuli), does there arise a complete reflex with both components.

But there is one more phenomenon which was observed by us in the course of other experiments in our laboratories and which manifested itself outside the hypnotic state; it would be opportune to analyse this phenomenon in the light of our present explanations. The dog eats the food, but no secretion of saliva is observed for ten or twenty seconds. This is undoubtedly due to the development of inhibition deliberately induced in the cortex by means of artificial conditioned stimuli for definite periods of time. How is this phenomenon to be interpreted? What mechanism is responsible for it? It must be assumed that an intense inhibition develops from the points of application of the artificial conditioned stimuli and spreads over the entire subcortical alimentary centre with both of its principal components—secretory and motor—as well as over the corresponding part of the cortical motor analyser.The moment food is presented, there arises at the points of application of the strongest natural conditioned stimuli, which have not participated in developing inhibition, an excitation rapidly affecting the alimentary region of the motor analyser; the latter is more labile in comparison with the subcortical centre, where the inhibition dissipates only if the motor effect of the uncondi-

tioned stimulus is more pronounced. One might draw a certain analogy between this phenomenon and the deliberate, volitional introduction of food into the mouth, its mastication and ingestion, in the absence of any trace of appetite.

However, it can, of course, be assumed (there are sufficient grounds for this) that the conditioned connection with the salivary secretion is likewise effected in the cortex through the cortical representation of the salivary glands, and if so, all the cases of dissociation of the secretory and motor reactions can be attributed to a different localization of inhibition at the onset of the hypnotic state and in the course of its development.

Another hypnotic phenomenon whose physiological mechanism must be elucidated by us is negativism. This, obviously, is a manifestation of inhibition, since it is a phasic phenomenon which gradually ends in sleep. Likewise, there is no doubt that it is a cortical localized inhibition because the salivary reaction accompanying it reveals a conditioned, i.e., cortical character. Consequently, it is natural to conclude that this is a motor inhibition related to the motor region of the cortex, to the motor analyser. But how is this form of inhibition to be explained? Why does the negative phase of the motor action appear first and the positive one next? What causes the change? It seems to us that this can be easily explained by more general, already known facts. When the hypnotic, inhibitory state sets in, the cortical cells become, as it were, weaker and less efficient—the maximum limit of their possible excitability diminishes. This is the so-called paradoxical phase, when a strong stimulus usually turns into a super-powerful one and may evoke not excitation, but inhibition, or it may strengthen the latter. We must also assume that a movement proceeding from the motor analyser, as is generally the case, consists of two opposite innervations—positive and negative, a movement towards the object and a movement from the object, which is similar to the relations of the flexors and extensors in the limbs. The negativism may be then explained in the following way.

A conditioned stimulus, slightly inhibited or not inhibited at all, directs a stimulation from the cortex to a corresponding positive innervating point of the motor region which is in a paradoxical state due to a certain degree of hypnotization. That is why the stimulation does not excite the above-mentioned point, but intensifies its inhibition. Then this extraordinary local inhibition, in accordance with the law of reciprocal induction, excites the negative point which is closely associated with the positive one. Hence the first negative phase of negativism. When the stimulus is removed, the extraordinarily inhibited positive point, by virtue of internal reciprocal induction, immediately becomes excited itself; at the same time the negative point, excited by the induction, at once passes into a state of extraordinary inhibition and in its turn positively induces the positive point. Thus, after its first extraordinary inhibition the positive point undergoes, so to speak, a double excitation. In accordance with this, if the hypnotic state does not deepen, the positive phase usually takes the upper hand after a single or repeated presentation and removal of the food—the dog begins to take it. We observe, then, a highly labile state of the cellular activity which is one of the properties of the transitional phase. This is proved by the further course of developments. If the hypnotic state deepens, there remains only the negative phase; reverse induction becomes impossible, and no excitation of the motor innervating apparatus is observed at all.

Approximately in this period of the conditioned alimentary reaction under hypnosis there is manifested one of the conditions for a fragmentary localization of hypnogenous inhibition in the cortex. One of our dogs, as shown in the descriptive part of this article, exhibited a very interesting and peculiar phenomenon (already mentioned by one of us in a previous article[96]). This relates to a definite sequence of inhibition in the adjacent zones of the motor region. The sequence can be explained by the fact that the inhibition embraces first of all those regions whose activity was most intense before the onset of the hypnotic state. Since in the

repeated process of eating the masticatory and lingual muscles worked most of all, then the cervical muscles, and finally the muscles of the trunk, the inhibition manifested itself in the same sequence.

The interesting phenomenon of a positive excitatory influence exerted in the course of hypnotization by the slightest change in the appearance of the food and in the manner of its presentation, is likewise accounted for by the general property of the cortical activity already known to us. It was established in our laboratory long ago (by Dr. Y. V. Volborth) that there is a conditioned inhibition of the second order, just as there is a conditioned excitation of the second order. The phenomenon is as follows. If an indifferent stimulus repeatedly coincides in time with an elaborated inhibitory process (for example, in the course of a differentiation), then it soon becomes an inhibitory agent itself. It is then easily understood why everything acting on the cerebral hemispheres during the state of hypnosis (which in itself is a certain degree of inhibition) acquires an inhibitory character. Hence, it is sometimes sufficient to bring the dog into the experimental chamber to evoke in it a hypnotic state. Any new stimuli, even very insignificant ones, naturally do not produce this inhibitory effect, and consequently, evoke positive cortical activity.

The auto-curative reflex mentioned in the descriptive part of this article is simply one of the subcortical reflexes manifested in the state of hypnosis after short feeding. The process of eating with all its exciting components, acts on the more or less hypnotized cortex as a strong stimulus and entails an intensification of cortical inhibition. A positive induction then proceeds from the cortex to the subcortical centres, which are now under the action of the ultra-weak stimuli or traces of former strong stimuli. The animal begins to sneeze, to scratch itself, etc., which was not observed in the alert state. Of a similar nature was the experimental case with a dog whose state resembled a war-time neurosis;

this case is described and analysed in the present volume of *Collected Papers*.[97]

As to the effect of differentiations, i.e., of conditioned inhibitory stimuli, we have long known that their influence on diffused inhibition is of a two-fold, contrasting character. In the case of a very feeble, diffused cortical inhibition, of a weak hypnotic intensity, the well-elaborated inhibitory stimulus concentrates the diffused inhibition to a greater or lesser degree and in doing so either fully abolishes the hypnotic state or weakens it. On the contrary, in the case of a strong inhibitory tonus of the cortex, the same stimulus intensifies the inhibition, as it were, by its summation with the existing inhibition. Consequently, the result is determined by the relations of intensity.

Let us, finally, consider the last experiment cited by us in the descriptive part of this article, when an extremely strong stimulus, contrary to stimuli of moderate strength and to weak stimuli, instead of intensifying the inhibition, often produced a positive action. The latter can be explained by the direct influence of the extremely strong stimulus on the subcortex; the intense subcortical excitation is communicated to the cortex, thus dissipating or weakening the inhibitory process in it. A special experimental method applied by us proves the correctness of this interpretation. When the monotonous experimental surroundings begin to have a hypnotizing effect on some of our animals, we, incidentally, counteract it by increasing their alimentary excitability by means of a certain diminution of their daily food ration. And naturally this increase of alimentary excitability must be located in the subcortical alimentary centre.

THE PROBLEM OF SLEEP[98]

Dear Comrades,

Although something extraordinary, one might say, even distressing, befell me yesterday, with the result that I am now, so to speak, not quite myself, I thought it necessary, nevertheless, to be present at the conference. Why? Because I believe that in a discussion of a scientific matter such as sleep, which is essential both from the practical and clinical points of view, my judgement will be not without interest, especially since I, jointly with my colleagues, have been studying the phenomena of sleep for thirty-five years in the course of our research into the higher nervous activity of dogs.

We came up against the phenomena of sleep at an early stage in our research; we were obliged to consider it, to subject it to special investigation, which now gives me the right to speak on this subject. That is why, despite my somewhat disturbed state, I decided to come here and to say a few words.

I

I should like first of all to make a general remark. The more perfect the nervous system of the animal organism, the more centralized it is, the more its higher part controls and regulates the entire activity of the organism, even though this is not clearly manifest. It might seem to us that

in higher animals many functions are effected independently of the influence of the cerebral hemispheres, but this is not so in reality. The higher part controls all the phenomena which develop in the organism. This was established long ago in the phenomena of hypnotic suggestion and auto-suggestion. It is well known that during hypnotic sleep it is possible to influence many vegetative processes by means of suggestion. On the other hand, we know of cases of auto-suggestion, such as symptoms of imaginary pregnancy, accompanied by an active state of the lacteal glands and the accumulation of fat in the abdominal walls, simulating the pregnancy. All this originates from the head, from thoughts and words, from the cerebral hemispheres in order to influence such a peaceful and genuinely vegetative process as the growth of the adipose tissue.

If the cerebral hemispheres, as everybody knows, are concerned with the slightest details of our movements, bringing some into action and suppressing others, just as it takes place, for example, when one plays the piano, one can easily imagine the minuteness of the degree of inhibition: one movement of a certain intensity is effected, while another, neighbouring movement, even the smallest one, is suppressed and retained. Or take, for example, our speech movements. What a multitude of words we have for expressing our thoughts! Nevertheless, we are precise in conveying the sense; we never use unnecessary words, employing only those which are most suitable in the given case, etc. Consequently, if the cerebral hemispheres constantly interfere even with these minute everyday activities and regulate them, it would be strange to suppose that the division of our activity into wakeful and sleeping states does not depend on the cerebral hemispheres. It is clear that here, too, supreme power belongs to the cerebral hemispheres and all of us are well aware of this.

Now, at a certain time of the day we become drowsy, and, since we are tired, sleep sets in. But we can do without sleep a whole night, and even for two or three nights

in succession. And it is our head, our cerebral hemispheres which, of course, control this phenomenon.

I shall now turn to the details.

It is clear, and everybody is aware of this now, since it has become a widespread and established physiological truth—that our entire nervous activity consists of two processes—excitatory and inhibitory—and that our whole life is a continuous interaction of these two processes.

When we began our objective study of the higher nervous activity by the method of conditioned reflexes, and began to elucidate the laws of the particular functions and tasks accomplished by the cerebral hemispheres, we, of course, immediately encountered the two processes. Every physiologist knows that these processes are inseparable, that they are always present not only in the nerve cell, but in each nerve fibre.

(I must make a certain reservation. If I begin to speak about conditioned reflexes this would take a lot of time, and I do not know when I would end. Since we have been working on conditioned reflexes for thirty-five years and have published the results of our work in special papers and books, allow me to assume that knowledge of conditioned reflexes is widespread and consequently, there is no need to treat this subject in an elementary way, i.e., to begin all over again.)

When we applied our conditioned stimuli and then carried out a detailed investigation of the activity evoked by them at every given moment, we constantly observed a spontaneous development of inhibition side by side with excitation. In other cases we produced the inhibition ourselves when we wanted to separate different phenomena.

Since you are acquainted to a degree with the conditioned reflexes, you undoubtedly know that we have, on the one hand, external stimuli which produce an excitatory process in the central nervous system, and, on the other hand, stimuli which produce an inhibitory process in the cerebral hemispheres. Right at the beginning of our research

we observed that as soon as we applied the inhibitory stimulus, a somnolent state of the animal, in the form of drowsiness or sleep, immediately intervened. This was of a constant character. We had to conclude, therefore, that these phenomena are closely interconnected and that certain efforts and resources are necessary to get rid of this drowsiness or sleep in the course of experimentation. Thus, when an inhibitory process arises in the cerebral hemispheres, establishing in them a certain differentiation either between the stimuli or between different moments of stimulation, etc., a state of drowsiness inevitably develops.

You can see, as we have seen during the past thirty-five years, that every time a cortical inhibition sets in which analytically assigns its proper place to everything, giving free rein to one process and suppressing the other, a state of drowsiness or in its ultimate stage of development—a state of sleep—simultaneously and invariably appears. The view that drowsiness and sleep are phenomena related to the cerebral hemispheres and that they are the result of the action of definite stimuli, is strictly obligatory for us. Surely a phenomenon observed every day is beyond any doubt.

That, of course, leads to the next question. How does this come about? What has this to do with sleep when it is simply a matter of differentiation between stimuli? They appear to be different things having nothing in common.

But the matter is quite simple. If we admit that everything can be explained by a constant interaction between the excitatory and inhibitory processes, then we shall have no difficulty in understanding the phenomena. Every time you produce an inhibition, a physiological inhibition, i.e., when you want to separate the active state from the inactive, drowsiness, as I have already said, immediately begins to manifest itself. But you can always eliminate this drowsiness, suppress it, and, on the contrary, ensure the predominance of the excitatory process. This is within your power, within your experimental possibilities, and it is what we do. The moment a state of drowsiness develops in the

dog during an experiment, i.e., the moment inhibition takes the upper hand, we apply a stimulation, thereby eliminating the drowsiness, limiting the inhibition and confining it within definite bounds.

How, then, is this matter to be further interpreted? It must be admitted that both excitation and inhibition are dynamic processes, which, on the one hand, may irradiate and spread, and, on the other, may be driven into definite narrow confines and concentrated there. This is the main point, the whole secret, and it is this that we use in all our physiological activity.

The basic property of both processes consists in the fact that on the one hand, when they arise, they tend to spread, to occupy an undue area; on the other hand, they can, given the corresponding conditions, concentrate in definite regions and remain there. When the inhibition is irradiated, diffused, you have the phenomenon of drowsiness or sleep.

Everybody knows, of course, that sleep does not set in instantaneously, that it is a gradual process. Similarly one does not awake all of a sudden; certain time is required before one gradually becomes active and, so to speak, completely throws off the fetters of sleep.

I advise everybody who values scientific truth, who does not want to reconcile himself to superficial knowledge, who is tormented by the thought "is this right or not?", to make a thorough study of two articles in my book *Twenty Years of Objective Study* which is the result of thirty-five years of intense reflections. One of the articles is entitled "Inhibition and Sleep" and the other, written jointly with M. K. Petrova,—"The Physiology of the Hypnotic State."

In any case, in order to give you a more or less clear illustration of this phenomenon, I shall cite one of our experiments.

I must tell you that when you observe the genesis of drowsiness and its first manifestations, you become convinced, and unshakably so, that hypnosis and sleep are, of course, one and the same process. In essence, hypnosis does

not differ from sleep; it differs only in certain peculiarities. Hypnosis, for example, is sleep which develops very slowly, i.e., it is at first confined to a very small and restricted area and then begins to spread farther and farther until it finally descends from the cerebral hemispheres to the subcortex, leaving untouched only the centres of respiration, of the heart-beat, etc., though somewhat weakening these too.

I shall now submit to you one of the numerous cases investigated by us in the course of thirty-five years. Let us take a dog which is falling into a state of drowsiness, sleep or hypnosis. What do we observe in this animal? Our experiments with conditioned alimentary reflexes show the following: at first the dog works and eats quite normally; then its tongue comes out of the mouth in a strange manner, and gradually begins to fall down. This is the first manifestation of a certain functional paralysis, of a diminution of activity, of inhibition of the minute centre in the motor region of the cortex which controls the movement of the tongue. This centre becomes inactive, as a result of which the tongue is paralyzed and falls out of the mouth.

A certain period of time passes, and you give the dog food. You see that its tongue functions very slowly and awkwardly; later, you also observe—not at once, but perhaps after the second or third offering of food—that the dog uses its jaws with difficulty, that its mastication is utterly impeded, since the mouth opens and closes very slowly. Thus you witness a weakening of the activity of the masticating musculature, its inhibition or sleep.

At the same time, however, you notice that when food is offered to the dog, which until then was standing with its head turned away or with its eyes fixed on the ceiling, it easily and quickly turns its head towards you and falls upon the food.

But as time goes on, you observe in the course of the experiment that although the dog turns towards you, it

brings its head to the food with great difficulty. Consequently, the inhibition or sleep has already seized other points of the skeletal movement, namely, those which control the movement of the neck.

You then see that the dog is unable even to turn towards the food, that it does not move the neck and does not take the food. And finally, you observe the onset of a general passivity of the skeletal musculature: the dog hangs limply in the loops, it is in a state of sleep. Thus, inhibition gradually develops before your eyes in a very obvious and concrete manner; at first it affects the tongue, then it spreads to the cervical muscles, from there to the general skeletal musculature until, finally, sleep sets in.

When you observe this development you can hardly doubt that inhibition and sleep are one and the same process.

The articles to which I have just referred contain numerous similar facts. And anyone that makes a thorough study of them will be convinced that inhibition and sleep are one and the same phenomenon. The only difference is that when the most minute points of the cerebral hemispheres are inactive, it is inhibition and, at the same time, sleep of an isolated cell; but when this inhibition, duly or unduly, spreads under the influence of certain conditions, it embraces more and more new areas of cells and is manifested in a passive, inactive state of the numerous organs dependent on these regions.

It is a pity that cinematography appeared too late and could not be utilized by us and our physiological laboratories. Had it been as accessible then as it is now, all these phenomena could have been very easily comprehended. We could now demonstrate them to you in the space of fifteen minutes, and you would leave us with the deep conviction that inhibition and sleep are one and the same process. But while inhibition is a concentrated process, hypnosis and sleep represent an inhibition which spreads over more or less vast areas.

This displacement of inhibition is of great importance for the comprehension of numerous nervous phenomena.

The British mind, as far as I have been able to follow it, has fully realized and caught up this idea. Thus, Wilson, one of the outstanding British neurologists, now considers all cases of narcolepsy[99] and cataplexy[100] precisely from this point of view. And we, who have observed all these phenomena in dogs, fully agree with him. In our opinion, Wilson is undoubtedly on the right trail.

Such, in general outline, of course, is our understanding of the phenomena relating to alternating sleep in the cerebral hemispheres, as well as to the sleep of the entire brain, following the mobile inhibition.

2

I shall pass now to other facts which to a certain degree compete with the concept just developed by me.

First of all I draw your attention to an extremely important fact recently obtained in the Soviet Union by Prof. Galkin, in A. D. Speransky's laboratory. It should be pointed out that this fact had been observed long ago in the clinic, but only once. Of course, much consideration was given to it at the time, and it was even properly understood by some researchers; but a single fact is not sufficiently convincing. This fact concerns an observation made long ago by Strümpel on a patient, in whom most of the sense organs were damaged and who could communicate with the external world only through two openings which remained intact—one eye and one ear. When he covered these openings with his hands he inevitably fell asleep.

This phenomenon is now being reproduced in the laboratory, and in the following way. We destroy three distant receptors in the dog, namely, smell, hearing and sight; this means that we section the fili olfactorii,[101] sever the n. optici or extirpate the eyes and damage both cochleae. After

this operation the dog sleeps twenty-three and a half hours a day. It wakens only when the elementary functions of the organism begin to annoy it—the necessity to eat, to evacuate the urinary bladder or the bowels, etc. But it is extremely difficult to awake the animal in the middle of the day. For this purpose it is not sufficient to stroke the dog, it is absolutely necessary to shake it; and then, before your eyes, it slowly awakens, stretches itself, yawns, and finally stands up. Such is the fact, and it is an exact fact. We repeated the experiment several times and the result was always the same.

The character of the operation performed on the dog excludes any supposition that its nervous system has been damaged. If the operation is done thoroughly, the dog comes through it more or less easily; the fact that two days after the operation it is able to eat shows best the ease with which it endures the loss of the above-mentioned receptors.

However, I must direct your attention to a minor detail. If you destroy the receptors gradually, i.e., at first one of them, the second two or three months later, and in another period of three months the third, then sleep does not set in. The dog, of course, is not as active as the animal which sees and hears normally; indeed, if it has lost the sense of smell and is unable to see, what can make it move? And it is perfectly understandable that for the most part it lies rolled up. But the moment you touch the intact receptor, for example, by stroking the dog, it immediately rises and begins to act.

When, however, you deprive the cerebral hemispheres of a large quantity of stimulations at once, the dog falls into a state of deep sleep. This indubitable fact, which must be reckoned with, naturally gives rise to the following question: how is this phenomenon to be interpreted? And in this connection there arises the problem of two kinds of sleep—the passive sleep caused by the abolition of a large quantity of stimulations usually reaching the cerebral hemispheres, and the active sleep which, in my understanding, is an in-

hibitory process, since the latter must be undoubtedly regarded as an active process and not as a state of inactivity.

Then the following question of principle arises: Does not the nervous system experience three different states—excitation, inhibition, and a certain indifferent state, when the first two are absent?

But proceeding from the general biological data we have grounds for doubting the existence of a neutral state. Life is a continuous interchange of destruction and restoration, in view of which a neutral state is simply inconceivable. On the whole, the problem can be reduced to the following: is not the passive sleep, which differs from the usual sleep developing under the above-mentioned conditions, also a result of active inhibition?

I think that certain considerations can be submitted which make it clear that the cases of sleep observed in dogs, operated upon in accordance with the method of Speransky and Galkin, could be also accounted for by inhibition; it is an active inhibition greatly favoured by the circumstances, since now there is no need for the inhibition to struggle against an extensive excitatory process and train itself, and as a result the stimulations falling upon the dog extremely facilitate the sleep. Why is this so? Because when the dog is mostly in a lying posture, certain points of its skin are continuously stimulated both mechanically and thermally. It is, therefore, conceivable that the passive sleep is evoked by a continuous and monotonous stimulation of the remaining receptors. And we know the fundamental rule according to which each cell, under the influence of continuous and monotonous stimulations, inevitably becomes inhibited. Consequently, it is possible to interpret this sleep as a result of inhibition proceeding from the remaining receptors subjected to a prolonged monotonous stimulation.

This is partially confirmed also by the following fact. When these dogs are transferred to new surroundings, they at first become more active, are wakened more easily, etc.; in other words, for a time they appear to be more lively.

It can be assumed, therefore, that here, too, due to a decline of the tonus, to the weakening of the excitatory process, the inhibition easily takes possession of the cerebral hemispheres and that weak, monotonous stimulations arise provoking an inhibitory process.

Then comes the following question: what happens to the dogs in which the cerebral hemispheres are extirpated? As a matter of fact, they, too, fall into a state of sleep. And this circumstance is often used as a serious objection to what I have just said, namely, to the statement that normally sleep originates in the cerebral hemispheres.

But I do not regard this objection as being physiologically grounded. It is clear that since sleep is a diffused inhibition, and the latter spreads over the nervous system up to the lower limit of the spinal cord, and since there is a central system and a nerve fibre, inhibition must indispensably take place. In cases when the cerebral hemispheres are absent, why should the inhibition not develop in the lower parts of the central nervous system, now in a concentrated, now in an irradiated form? This is all the more likely since dogs possess lower levels of distant receptors—corpora geniculata[102] (one relating to the ear, and the other—to the eye), and we know that a dog deprived of the cerebral hemispheres reacts to acoustic and visual stimuli. Consequently, the conditions remain the same as when the cerebral hemispheres are intact, and sleep in this case is not excluded—it must inevitably manifest itself. So long as there exists inhibition and there is a cell which, as a result of excitation, is bound to become fatigued and fall into a state of inhibition, all the conditions for the development of inhibition are present. But in the absence of the cortex sleep begins from the subcortical formations. Hence, there is no contradiction here as far as the fundamental facts are concerned, that is, the interchange of excitation and inhibition, their concentration and irradiation. If all these phenomena take place also in the lower part of the central nervous system, then why should sleep not develop there as well? Therefore, I

regard these objections as being physiologically groundless; they cannot refute our statement about the initiative of the cerebral hemispheres in the development of sleep in normal conditions.

Next come more important facts. On the one hand, a clinical fact—the encephalitic sleep[103] or somnolence, and on the other, the physiological apparatus advanced by the Swiss physiologist Hess, which, as it were, rivals my concept about sleep originating in the cerebral hemispheres.

As for clinical sleep, the clinical concept of the centre of sleep is well known to clinicians; it is based on the fact that after an infection of the brain, the so-called encephalitis, which is accompanied by somnolence, considerable changes take place in the hypothalamus.[104] On the basis of this fact the simple conclusion is made that the centre of sleep must be located there.

However, I make bold to say that this reasoning, which is based on the fact that there is, on the one hand, sleep, and on the other, a destruction of the hypothalamus, is oversimplified. The above conclusion is, therefore, too hasty.

Firstly, all that we know about the work of the cerebral hemispheres makes the concept of the hypothalamus as the actual centre of sleep doubtful and incomprehensible. It is difficult to assume that an infectious process arising in the brain should in no way tell upon its most reactive part—the cerebral hemispheres. It is likewise difficult to assume that the toxins should remain exclusively in the subcortex, without spreading to the cerebral hemispheres. I fully realize, of course, that bacteria favour definite chemical media, and that there must be a very delicate difference between the above-mentioned parts of the brain in respect of their chemical composition. It is quite conceivable that this is true, that the process in question concentrates mainly in the hypothalamus and produces in the nerve cells changes which can be afterwards revealed microscopically. But it may be that in the cerebral hemispheres these changes have only a functional character and manifest themselves in the weak-

ening of the excitability of the hemispheres; at the same time they may be inaccessible to microscopic investigation. It can be supposed that there is a certain gradation of the patho-anatomical changes—from visible phenomena to purely functional, and, finally, invisible ones.

On the basis of what we observe in the hypothalamus it is difficult to assert with confidence that these infections do not exert any influence on the cerebral hemispheres. I would regard such a conclusion as being too hasty.

Secondly, I do not contest the fact that encephalitis is accompanied by sleep, and that this phenomenon is related to the hypothalamus and complies with it. However, I am inclined to interpret this fact in the same way as I have done with regard to the fact established by Speransky and Galkin. Here is what I have to say in this connection. There is no doubt that the hypothalamus is a wide route with definite centres where the stimulations coming from the internal world, i.e., from all the internal organs, are accumulated; its destruction leads to the isolation of the cerebral hemispheres from the entire internal world, from the entire activity of the organs; in other words, it provokes a state analogous to that which arises when all three receptors are destroyed, i.e., when the cerebral hemispheres are deprived of external stimulations. The stimulations proceeding from the internal organs, although we are not conscious of them, constantly maintain a heightened tonus of the cerebral hemispheres. This is proved in the first place by the fact that, as I have already mentioned, dogs with extirpated cerebral hemispheres are in a continuous state of sleep. Further proof is provided by a pigeon deprived of the cerebral hemispheres and remaining constantly immobile and somnolent. But the moment there arises the necessity to eat or to evacuate the excretory organs, the pigeon awakes. Consequently, there is no doubt that these stimulations act on the cerebral hemispheres and bring it to a state of wakefulness.

On the other hand, we know very well that in certain,

particular cases, we feel the heart-beat, the movements of the intestines, etc.

Another long-established fact shows that internal stimulations contribute to the maintenance of the cortex in an alert state, to its tonus. This fact was recently confirmed in America, in laboratory conditions, on a person in whom the ability to resist sleep for a long time was investigated. The following phenomenon was observed. A person who like yourself is interested in this particular investigation and who tries hard to keep awake as long as possible, despite a strong desire for sleep, successfully resists the state of somnolence only when he walks or when he is in sitting posture. The moment he lies down, i.e., relaxes the musculature, he immediately falls asleep.

Thus you clearly see that our internal stimulations greatly contribute to the maintenance of a certain tonus in the cortex.

In my view, encephalitic sleep is caused by the separation of all internal stimulations from the cerebral hemispheres due to an affection of the hypothalamus; it is, consequently, the same drastic decline of the tonus that is observed when the external receptors are destroyed.

There remains one more important fact which supports the reasoning of the clinicians concerning the centre of sleep. I have in mind the experiments of Hess, in the course of which sleep was evoked by electric stimulation of definite parts of the brain. I am not going to contest this fact either. I fully admit it and believe that it will be reproduced by other investigators; but I consider it necessary to say a few words about its proper interpretation and the objections which can be raised to the conclusion drawn by Hess.

The first thing which attracts attention is that the above fact does not fully accord with the clinical fact, since the points in the latter case do not coincide with those stimulated by Hess.

Hess himself emphasized this circumstance and stated that his experiments would disappoint the clinicians, since

anatomically the points which produced sleep did not coincide.

Whereas the lesions caused by encephalitis are located in the region of the third ventricle, in its lateral walls, etc., Hess subjected to stimulation the lowest part of the brain, almost reaching the brain stem.

How is this fact to be interpreted? It must be pointed out that a phenomenon observed in the given organism under normal conditions, as in our case, is one thing, and a phenomenon observed under pathological conditions, especially when they are artificially produced in the laboratory, as for example, the stimulation of the brain, is another thing. They are, of course, absolutely different phenomena. While in the latter case maximum simplicity can be attained, in the normal state the phenomena become complicated. But in the given case even Hess, who obtained a definite state in dogs by stimulating certain points in the brain, stated that this could be an excitation not only of the cells of an imaginary, fantastic "centre of sleep," but of centrifugal or centripetal fibres; at the same time he drew attention to the fact that the points used by him for producing a state of sleep had been very limited.

Then I am fully entitled to ask the following question: is not this simply a reflex sleep originating from the same cerebral hemispheres? Indeed, we know very well that a monotonous irritation of the skin, both in our laboratory experiments on dogs and in our experiments on human beings, produces a hypnotic state, a state of sleep. There is nothing surprising in the fact that certain stimulations of the nerve paths may provoke sleep. Consequently, these experiments do not prove that sleep is a stimulation of a definite centre. Along with hypnotization by means of passes, which, undoubtedly, is a reflex inhibition caused by monotonous stimulations, a hypnotic state can also be evoked with the help of the verbal method. The latter is addressed to the cerebral hemispheres. In our laboratory we produce a state of sleep in dogs by means of a weak electric

stimulation of the skin; this sleep is so persistent that after several experiments the place where the electrodes were fixed becomes a conditioned hypnogenous stimulus: it suffices to touch this place or to cut the hair on it, and the dog immediately subsides into deep sleep. Such is the effect of peripheral stimulations.

What, then, is the value of Hess's proof, especially since he himself states that his sleep is produced with the help of a weak electric current, and besides, a special (faradic, and not direct) one? Consequently, this could be a very weak stimulation corresponding to that which we obtain in the laboratory by means of a weak electric current.

I find, therefore, that the Hess experiment, which was so highly convincing in the eyes of the author himself, and even more so in the eyes of the clinicians, can be rightfully contested and reduced to what I have already said, the existence of a special centre of sleep being out of the question. In my opinion, the crude idea that there is a special group of nerve cells which produce sleep, while another group produces the state of wakefulness, is, from the physiological point of view, contradictory. We observe the phenomenon of sleep in every cell; what reason have we, then, for asserting that there is a special group of hypnogenous cells? If a cell exists, it inevitably produces a state of inhibition, which irradiates and renders all the neighbouring cells inactive; and when the inhibition continues to spread, it produces sleep.

Such is my firm conviction.

DISCUSSION

Question: What is responsible for the absence of sleep in dogs whose distant receptors were extirpated at different times?

Answer: As you know, the inaction of one receptor always leads to an intense training of all others. It is a well-known

fact, for example, that blind people have a highly sensitive touch. The same thing occurs in the given case with the reception of the external world when the olfactory receptor is removed; the activity of the latter is made up by the reinforced activity of the ear or the eye. It is, therefore, obvious that successive extirpation of the receptors makes possible such a training, while simultaneous extirpation excludes it.

It should be pointed out that there are indications which show that with the lapse of time, in the course of years, the dogs to a certain extent train themselves with the help of the remaining receptors (that is, of the oral and cutaneous receptors) and in the end become more active. In any case this fact was manifested in dogs which have been used for these operations.

Question: From the point of view of inhibition how do you explain a sleep accompanied by abundant dreams?

Answer: As I have already said, sleep is an inhibition which gradually and steadily spreads to the lower levels of the brain. It is clear, therefore, that when sleep and fatigue begin to set in, the highest part of the cerebral hemispheres, which controls verbal activity (I call it the second signalling system of reality), becomes inhibited first, since we constantly operate with words.

I can add now—for the sake of brevity I omitted it in my talk—that this inhibitory process has its external and internal stimuli.

Among the internal stimuli of inhibition is the humoral element, or consequently, certain cellular metabolites, which evoke this inhibition. On the other hand, the external inhibitory stimuli, as I have already mentioned, are monotonous and weak. Naturally, it is the highest part of our brain, the verbal part of our higher cortical activity which functions in the daytime. Fatigue calls forth inhibition, and this part becomes inactive. But along with the verbal function of the cerebral hemispheres there is a function which

we share with animals and which is termed by me the first signalling system, i.e., the reception of impressions produced by all the stimuli acting on us.

It is quite clear that when we are in an alert state, the part of the cortex controlling our speech, inhibits the first signalling system; that is why in the alert state we (except the artistic type of man whose constitution is of a peculiar character), when speaking, never imagine the object which we designate by words. I close my eyes and think of the person sitting in front of me, but I do not see him in my thoughts. Why? Because the excitation of the higher part inhibits the lower part. That is why when sleep begins and embraces only the higher part of the hemispheres, the adjacent lower part bearing a direct relation to impressions prevails and is manifested in dreams. When there is no pressure from above, a certain degree of freedom sets in. And even here a new fact must be added, a fact encountered in physiology, namely, positive induction. When one point becomes inhibited, the other, on the contrary, becomes excited. And if we grant this, i.e., if we assume positive induction, the phenomenon of sleep becomes particularly clear.

Question: Judging by what you have said, there is no centre of sleep. How, then, are we to explain the fact that for such an important function as sleep there is no centre, while there are centres for other, even less important functions of metabolism, for example, a sugar centre, a water centre, etc.?

Answer: The explanation is quite simple. Inhibition and sleep exist for each cell. Consequently, they do not need any special cellular groups.

Question: How should the problem of fatigue be considered from this point of view?

Answer: I have already said that fatigue is one of the automatic internal stimuli of the inhibitory process.

Question: How do you explain the occurrence of fits during sleep?

Answer: There is nothing special in this, because we are aware of the resources of our nervous system, the cerebral hemispheres. The following phenomenon is often observed: inhibition spreads over the cerebral hemispheres and sleep sets in; nevertheless, certain points, which I call points on duty or on guard, may remain active. This is observed, for example, in the sleep of the miller who wakes up when the noise of the mill ceases, or in the sleep of the mother who wakes up at the faintest sound coming from her child, but who is not disturbed by much louder sounds. So that when the conditions for the excitation of a certain part arise, sleep does not prevent the development of the process.

Question: How can all the complicated reactions of a hypnotized person be explained, if we admit that in the state of hypnosis his entire nervous system is inhibited with the exception of the one point by means of which he communicates with the hypnotist?

Answer: I have pointed out that hypnosis is a kind of sleep which gradually spreads from a basic point.

Here is a fact which was observed in our laboratory. You have a dog which long ago was deprived of three receptors and which is in a constant state of sleep. Nevertheless, you can awaken it with the help of the remaining cutaneous receptors, bring it to the laboratory, place it in the stand and perform experiments on it. Then the following, extremely interesting phenomenon is observed, a phenomenon analogous to the hypnotic state: you can elaborate only one reflex in a dog of this kind; it is impossible to form in it, as can be done in a normal animal, two, three, or four reflexes simultaneously. This is explained by the fact that the cortical tonus, i.e., the excitatory process in the entire cortex, is very weak; hence, when it concentrates on one

stimulus, there is nothing left for other stimuli and they remain inactive.

In this way I explain also hypnosis and rapport.[105] The cerebral hemispheres are not wholly embraced by inhibition, since certain points of excitation may be formed in them. Through such an excited point you evoke a response and suggest. And then the hypnotized person inevitably executes your order, for when you give it you have everything extremely restricted. Consequently, all the influence of the other parts of the cerebral hemispheres on that which is suggested by your words, on the stimulations which you produce, is fully isolated from all others. And when the hypnotized person wakes up after such suggestion, he is powerless to do anything with this isolated excitation, since it is detached from all others. Therefore, in hypnosis it is a question not of complete, but of partial sleep. That is the difference between hypnotic and natural sleep. Whereas natural sleep represents a general inhibition of the cerebral hemispheres, however, with the above-mentioned exception of the so-called points on duty and points on guard, hypnosis is a partial inhibition affecting only a definite point, all others remaining in an active state.

Question: How do you explain the regular interchange of sleep and wakefulness?

Answer: It is clear that our daytime activity is the sum total of the excitations which cause a certain amount of exhaustion; when this exhaustion reaches peak, it evokes automatically, in an internal humoral way, a state of inhibition accompanied by sleep.

IX

PHYSIOLOGY AND PSYCHOLOGY

PHYSIOLOGY AND PSYCHOLOGY IN THE STUDY OF THE HIGHER NERVOUS ACTIVITY OF ANIMALS[106]

First of all, I consider it my duty to thank the Philosophical Society for expressing readiness, through its chairman, to listen to what I have to say. I do not know to what extent my subject will be of interest to the members. As for myself, however, I have a special purpose which will be revealed at the end of my address.

I wish to inform you of the results of very extensive research carried out by me in the course of many years jointly with a dozen or so colleagues who constantly participated in it both with their heads and with their hands. Without their co-operation, this work would have been only one-tenth of what it is. So that when I use the word "I," you should take it not in the narrow sense of an author, but, so to speak, in the sense of a conductor. In the main I guided and co-ordinated the work.

Now for the essence of my subject.

Let us take any higher animal, for example, the dog. Although it is not at the top of the zoological ladder (the monkey occupies a higher place), it is closer to man than any other animal and has been his companion since prehistoric times. I heard the late zoologist Modest Bogdanov[107] state the following when reviewing prehistoric man and his companions, especially the dog: "Justice compels us to say that it is the dog that helped man to become what he is." Such is his appreciation of the dog. Consequently,

the dog is not just an ordinary animal. Indeed, consider a watch-dog, a hunter, a domestic pet, etc.; before your eyes is their entire activity in all its higher manifestations, or, as the Americans are inclined to call it, their entire behaviour. If I wished to study this higher activity of the dog, that is, to systematize the phenomena of its life and to disclose the laws and rules which govern them, the following question would inevitably confront me: how shall I act and which way shall I choose? Generally speaking, there are two ways. One is the ordinary way taken by everybody. It consists in attributing the human inner world to the animal, that is, in assuming that the animal thinks, feels, desires, etc., in much the same way as we do. Consequently, this means guessing what takes place within the animal and interpreting its behaviour on the basis of these suppositions. The other way is entirely different; this is the way of natural science which considers the phenomena, the facts, from a purely external aspect, and which in the given case would concentrate only on the agents of the external world that act on the dog, as well as on the visible reactions of the dog to these agents.

The question, therefore, is this: which way is preferable, more expedient, the best way to tackle the problem? Allow me to answer this question, which is of great importance, by presenting the facts in chronological order. Several decades ago my laboratory made a study of the digestive process, and specially investigated the activity of the digestive glands producing the digestive juices by means of which the food is transformed, assimilated by the organism and enters into the vital chemical processes. Our job was to study all the conditions which determined the work of these glands. Much of our investigation was devoted to the first of these glands, the salivary gland. A detailed and systematic study of the latter demonstrated that its work is extremely delicate and highly adaptable to whatever substance enters the mouth, and that the quantity and quality of saliva show corresponding considerable fluctuations.

When the ingested food is dry, there is an abundant secretion of saliva, since the food must be moistened; when the food is moist, the amount of saliva is smaller. If it is a matter of food which must pass into the stomach, the saliva is rich in mucus; it lubricates the mass of food and thereby facilitates its ingestion. But when there is a substance which must be ejected from the mouth, the saliva is watery and helps to rinse the mouth.

Thus, we see a series of delicate co-ordinations between the work of the salivary gland and the kind of substance upon which the saliva is secreted. Next comes the question: what underlies this delicate co-ordination, what is its mechanism? The physiologist—and that is my speciality—has a ready answer to this question. The properties of the food act on the nerve endings and stimulate them. These nervous impulses proceed to definite points of the central nervous system and thence to the nerves leading to the salivary gland. In this way there arises an obvious connection between the substance which enters the mouth and the work of the gland. The particulars of this connection are explained by the fact that the nerves from the oral cavity, where the substances act, are separately excited by acid, sweet, rough, soft, hard, hot, cold, etc.; thus the impulses are conducted now by one nerve, now by another. In the central nervous system these impulses are transmitted to the salivary gland along different nerves. Some of them evoke one kind of activity, the others—activity of a different kind. Consequently, the different properties of the food stimulate different nerves, and in the central nervous system there takes place a transfer of the impulses to corresponding nerves which evoke one or another activity.

Since we aimed at a complete investigation we had to consider all the concomitant conditions, apart from those I have just mentioned. The substances introduced into the mouth act on the salivary gland. But does the same thing occur when food is placed in front of the dog, i.e., is it effective at a distance? We know very well that when we

are hungry, the sight of food evokes in us a flow of saliva. Hence, the expression "the mouth waters." It was, therefore, necessary to investigate this phenomenon as well. What does it mean, especially since in this case there is no contact with food substances at all? Concerning these facts physiology used to say that in addition to ordinary stimulation, there is psychical stimulation of the salivary gland. Very well. But what does this imply, how is it to be interpreted, how must we, physiologists, tackle the question? We could not ignore it, because it played a certain part. On what grounds could we discard it? Let us, first of all, consider the bare fact of psychical excitation. It turned out that psychical excitation, i.e., the action of a substance at a distance, is exactly the same as when it is in the mouth. It is absolutely the same in all respects. Depending on the kind of food shown to the dog, whether dry or liquid, edible or absolutely inedible, the salivary gland functions in exactly the same way as when these substances are introduced into the mouth. The psychical excitation reveals exactly the same relations, but on a somewhat smaller scale. How, then, is this to be studied? Naturally, when we see a dog eat rapidly, snatch the food and chew it for a long time, we think, willy-nilly, that the animal strongly desires to eat, and that it is this that makes it rush to the food and swallow it. It longs to eat. Another time the dog's movements are slow and languid, and so we say that it has no great desire to eat. When it eats, you see the work of the muscles alone, which is fully aimed at introducing the food into the mouth, at chewing and swallowing it. Judging by all this, one would say that the dog experiences pleasure in eating. On the contrary, when an inedible substance gets into the mouth, and the dog ejects it, forces it out with the help of the tongue and by shaking its head, we involuntarily say that this is unpleasant for the animal. Now, when we decided to elucidate and analyse this phenomenon, we at first adopted this trite point of view. We took into account the feelings, desires, imagination, etc., of our animal. And

this resulted in a quite unexpected and extraordinary fact: one of my colleagues and I irreconcilably differed in opinion. We could not come to agreement, could not convince each other as to who was right; for decades prior to this, as well as afterwards, we always reached agreement on all questions, one way or another, but the given case ended in complete discord. This made us meditate on the matter. It seemed probable that we were not on the right track. And the more we thought about the matter, the more convinced we became that another course of investigation should be followed. Overcoming the difficulties which I experienced in the beginning, and taking the way of persistent thought and concentrated attention, I finally reached the ground of true objectivity. Such psychological expressions, as the dog guessed, wished, desired, etc., were wholly withdrawn from our use (in our laboratory a fine was even imposed on their use). Finally, all the phenomena with which we were concerned appeared to us in a new light. So what, then, is the point? What is that which the physiologists term psychical stimulation of the salivary gland? We, naturally, put ourselves the question: is it not a form of nervous activity, long ago established by physiology and well known to physiologists? Is it not a reflex? And what does this reflex of the physiologist represent? It consists of three chief elements. In the first place, there is an indispensable external agent producing the stimulation. In the second place, there is a definite nervous path by means of which the external impulse makes itself felt in the effector organ. This is the so-called reflex arc, a chain composed of an afferent nerve, a central part and a centrifugal or efferent nerve. And finally, in the third place, the law-governed, but not accidental or capricious, nature of the reaction. Given certain conditions the reaction always and invariably arises. Of course, this must not be understood in the sense of absolute constancy, in other words, as meaning that circumstances may never occur in which the agent does not act. It is obvious that there can be conditions in which the action remains

disguised. According to the law of gravity all things must fall to the earth, but once you support them this does not occur.

Now let us return to our subject. What, then, is the psychical stimulation of the salivary gland? When food is placed in front of the animal, before its eyes, it certainly acts on the animal, on its eye, ear, nose. No essential difference between this action and that in the mouth is observed. There are reflexes from the eye and from the ear. Upon hearing a loud sound we start—a reflex action. Under the action of a strong light the pupils of our eyes contract. Hence, this does not interfere with our concept that what we call psychical stimulation is a reflex. The second element, the nervous path, is, obviously, also present here; for when the dog sees the food, the nervous path originates not from the nerves of the mouth, but from those of the eye, then proceeds to the central nervous system and from there puts the salivary gland into action. Again there is no essential difference here, and there is nothing to prevent us from representing this as a reflex. Let us now examine the third element—the law-governed nature of the reaction. In this respect the following must be pointed out: the given stimulation cts less regularly, less often than when the stimulant is in the mouth. However, it is possible to acquire such a degree of knowledge and mastery of the subject that all the conditions on which the action of the substance at a distance depends will be under our control. If this has been attained (which is now the case), then the law-governed nature of the reaction is in evidence.

But the "psychical" excitation has an additional feature. When we examine these phenomena more closely, it appears that among the agents acting at a distance there may be some which did not exist previously. Here is an example. Let us suppose that the attendant enters for the first time the chamber in which the dog is kept and brings in the food. The food begins to act the moment it is shown to the dog. And if the same attendant brings in the food several days

in succession the upshot will be that the moment he opens the door and puts his head in, the action begins. Thus, a new stimulating agent has appeared. If this is continued long enough, then the sound of the attendant's steps alone is sufficient to evoke a secretion of saliva. Consequently, stimuli that did not exist before have now developed. The difference appears to be considerable and essential: while in the physiological stimulation the stimuli are constant, here they are variable. However, this point can be interpreted in the following way: should the new stimulus become effective under strictly definite conditions, which also can be determined by the experimenter, i.e., if the entire phenomenon obeys certain laws as well, then this cannot serve as an objection. Although the stimuli are new, they inevitably arise under definite conditions. Accident is ruled out. Here, too, the phenomena are related to definite laws. I can say that just as the first reflex was characterized by the presence of a stimulus which travelled along a definite path and, in certain circumstances, evoked our phenomenon, here, too, the phenomenon arises under strictly definite conditions. The essence, the composition of the concept of reflex has not changed in the least.

It has been proved that any agent of the external world can be made a stimulus of the salivary gland Any sound, odour, etc., may become a stimulus that will excite the salivary gland exactly in the same way as it is excited by food at a distance. As to the precision of the fact, there is no difference whatever; it is only necessary to take into account the conditions in which the fact exists. What, then, are the conditions which can become stimuli of the salivary gland? The chief condition is coincidence in time. The experiment is performed as follows. We take, for example, a certain sound which has no relation to the salivary gland. This sound acts on the dog. Then we feed the dog or introduce acid into its mouth. After several repetitions of this the sound itself, without the addition of food or acid, begins to excite the salivary gland. There are altogether four or five,

at most six conditions under which any stimulant, any agent of the external world, becomes a stimulus of the salivary gland in the dog. Once this is so, once it has become a stimulus under a definite series of conditions, it will always act with the same precision as food or as any rejected substance introduced into the mouth. If any external agent invariably becomes a stimulus of the salivary gland under definite conditions, and having become such, inevitably produces its action, then what grounds are there for saying that in essence this is anything other than a reflex? Actually this is a law-governed reaction of the organism to an external agent effected through the medium of a definite part of the nervous system.

As I have said, the ordinary reflex is formed like this: there is a definite nervous path along which the stimulation proceeding from the peripheral part is conducted to the effector organ, in the given case, the salivary gland. This conducting path is, so to speak, a live wire. But what happens in this case? Here it should be added that the nervous system is not only a conducting apparatus, as is generally regarded, but also a connecting one. And there is nothing paradoxical in this supposition. If in everyday life we widely use contactors, for example, in electric lighting, telephone communication, etc., then it would be strange indeed if in the most perfect machine on earth, there were no application of the principle of connection, but only of conduction. Hence, it is quite natural that along with conducting properties the nervous system should also possess a connecting apparatus. Analysis has shown that the constant form of stimulation of the salivary gland by food at a distance, which is an ordinary case known to everybody, is a similar formation of a new nervous path by means of connection. While working in Prof. Vartanov's laboratory, Dr. I. S. Tsitovich performed the following interesting experiment. He took a new-born puppy and fed it exclusively on milk for several months; the puppy had known no other food. Then he subjected it to an operation so that the work of the sali-

vary gland could be observed; afterwards he showed the puppy foods other than milk. But not one of them, shown at a distance, produced any effect on the salivary gland. Consequently, when different foods act from a distance, this is a reflex newly formed as a result of individual life experience. The matter can be described thus: when a piece of meat is first placed before a puppy several months old neither its appearance nor its odour produce any action on the salivary gland. It is necessary that the food be taken into the puppy's mouth at least once, to evoke a simple, purely conducting reflex, and only then there develops a new reflex to the appearance and odour of the meat. And so, gentlemen, you see that we have to recognize the existence of two kinds of reflexes: one is ready from the time of birth, and is of a purely conducting character, and the other is continuously, incessantly being formed in the course of individual life, obeys exactly the same laws, but rests on the basis of another property of our nervous system—on connection. The first reflex can be termed inborn, and the other—acquired, or respectively—generic and individual. The inborn, generic, constant and stereotyped reflex we termed unconditioned; the other, which depends on a multitude of conditions and constantly fluctuates in conformity with the circumstances, we called conditioned; in this way we characterized the reflexes from the standpoint of practice, from the point of view of laboratory investigation. The conditioned reflex is also indispensable, and thus, like the unconditioned reflex, belongs entirely to the domain of physiology. With this formulation physiology, of course, comes into possession of an enormous mass of new material, since there is an infinite number of these conditioned reflexes. Our life consists of a multitude of inborn reflexes. When one says that there are three kinds of these reflexes, namely, the self-defensive, the alimentary and the sexual, this is undoubtedly only an academic scheme; actually, they are numerous, and they must be divided and subdivided. Consequently, there is a multitude even of these simple, inborn reflexes; as for

conditioned reflexes their number is endless. And so, with the establishment of this new concept of conditioned reflex, physiology acquires a vast domain for investigation. This is the domain of higher activity connected with the higher centres of the nervous system, while the inborn reflexes are related to the lower part of the central nervous system. If you extirpate the cerebral hemispheres of an animal, the simple reflexes remain, but the new, connecting reflexes disappear. It is understandable that innumerable questions will arise in connection with these conditioned reflexes, if you constantly consider all the conditions under which they originate, exist, are disguised, temporarily weakened, etc. This is one half of the higher nervous activity, as the modern physiologist sees it. Now for the other half.

It is quite obvious that the nervous system of an animal consists of a set of analysers which decompose nature into its separate elements. We are familiar with physical analysers, for example, the prism which decomposes white light into its elementary colours, or resonators which split complex sounds into their component elements. The nervous system is a true collection of such analysers. The retina,[108] for instance, discerns in nature the oscillations of light; the acoustic part of the ear detects the oscillations of air, etc. Each of the analysers, in its turn, continues this endless division into separate elements. By means of our ear analysers we divide tones according to their wave lengths, wave amplitudes and forms. Thus the second function of the nervous system consists in analysing the external environment, in decomposing the different complexities of the world into their separate elements. This analysis is also effected by the lower parts of the central nervous system. If we decapitate an animal, with the result that its organism possesses only the spinal cord, the analysing function still remains. The moment you act mechanically, thermally or chemically upon such an animal, it reacts with a special movement to each stimulation. The most delicate analysis, of which both animal and man are capable, takes place in the higher

parts of the nervous system, in the cerebral hemispheres. And this is also a purely physiological subject. As a physiologist, there is no need for me, when studying this subject, to resort to any notions and concepts alien to physiology. Investigation of the analysers which are situated in the cerebral hemispheres discloses very important facts. For example, when a new reflex is formed from a certain sound for the first time, the new stimulus usually appears in a general form, i.e., if you have elaborated a conditioned reflex from a definite tone, say of 1,000 oscillations, and then try for the first time some other tones, for example, of 5,000, 500 or 50 oscillations, you will obtain an action from each of them as well. In the beginning the analyser always enters into the reflex with its more general function. Only later, with the repetition of the reflex, specialization gradually takes place. This is a very important law. It is clear that we can investigate this fact, too, without having recourse to concepts alien to physiology. The limit of the analysing capacity is likewise accessible to investigation. It has been proved, for example, that the analyser of the dog can discern one-eighth of a tone. The sensitiveness of the auditory apparatus of the dog for tones is much greater than that of man. While we are able to distinguish a maximum of 50,000 oscillations per second, the auditory apparatus of the dog is excited by a frequency of even 100,000 oscillations. In this connection I wish to remind you of the following interesting fact. If we damage the cerebral hemispheres, where the corresponding ends of the visual, auditory and other analysers are situated, there take place, of course, definite disorders. For example, when the terminals of the eye analyser of a dog are damaged, the animal does not recognize its master, but at the same time it does not blunder into a chair, or run into its master. That is why it has been said that the dog sees, but does not understand. It must be admitted, however, that this phrase, when analysed, is in itself difficult to understand.

When, in the given case, it is said that the dog sees but

fails to understand, this simply means that the analysing apparatus is damaged to a degree that reduces the analysing capacity to the minimum. The eye merely distinguishes light from shadow, and a space occupied by a body from a free space, but it is no longer able to distinguish the forms and colours of objects.

Thus, we recognize two aspects of the higher nervous activity in the higher animals: on the one hand, the formation of new connections with the external world, and on the other, the higher analysis of phenomena.

If you grant these two kinds of activity, you will see that they embrace a good deal, and it is difficult to assume that some things remain outside of them. Only a detailed study can establish this. All kinds of training, education, development of habits, orientation in the surrounding world, with its events, natural phenomena and people, represent either a formation of new connections, or the most delicate analysis. At least, very many manifestations are related to these two activities, and in any case limitless work awaits us in this field; but we, physiologists, in performing this work, do not make use of any alien concepts.

The study of the above-mentioned activities revealed that the first important property of the higher cerebral mass is a peculiar movement of the nervous processes in this mass. I shall not dwell on this now, since it will be the object of a separate experiment, of which I shall speak later and which I will describe in detail. Another extremely important property is that if in the higher part of the brain, in the cerebral hemispheres there is a certain functionally isolated element and if the latter is subjected to a constantly repeated stimulation proceeding from a definite agent, then, sooner or later, it inevitably falls into a state of inaction, a state of sleep or of hypnosis. The fundamental property of the higher nervous element is its extremely high reactivity; however, when it is temporarily isolated in such a way that the excitation does not spread, but concentrates in it for some time, i.e., if the excitation invariably acts on one

point, then this element always passes over into a state of sleep. This relation of the higher nervous cells to the stimuli explains many things. It can be interpreted either as a protection of the precious substance of the cerebral hemispheres, a substance which must constantly respond to all the influences of the external world; or it may be understood in the biological sense, i.e., when it is variable, the stimulus determines a reaction in the form of a definite activity, and when it becomes monotonous and remains without further consequences, it ensures rest and the possibility of preparing for a new expenditure. I shall not go into the details.

I am coming to the end of my talk. I shall describe an experiment which partly illustrates the data I have mentioned. I am most anxious to have your views about this fact, about this experiment. If some of my descriptions are not clear, interrupt me right away and ask for additional explanations in order to understand the entire experiment as clearly as if you had been present yourselves.

Here is a diagram illustrating the animal used for the experiment. For the present you see two black spots, one on the foreleg and the other on the thigh. These are the places to which we attached our apparatus for mechanical stimulation of the skin. We used the apparatus in the following way. When we put it into action and the mechanical stimulation of the above-indicated places began, we introduced acid into the dog's mouth. The acid, of course, evoked the secretion of saliva through a simple inborn reflex. This was repeated several times in succession, from day to day. After some repetitions the flow of saliva could be obtained merely by mechanical stimulation of the skin, just as if the acid had been introduced into the dog's mouth, although no acid was used.

Now I pass to the discussion of this fact, physiologically and, at the same time, as far as I can, psychologically, from the standpoint of the zoopsychologists. I cannot guarantee that I shall reproduce their phraseology, since I have lost

the habit of using their language, but I shall try to approximate to their expressions. The fact consists in the following. I apply a slight mechanical stimulation of the skin and then immediately pour acid into the dog's mouth. A simple reflex evokes the secretion of saliva. After this procedure has been repeated several times, a flow of saliva could be obtained by mechanical stimulation of the skin alone. Our interpretation of this phenomenon was that a new reflex had been formed, that a new nervous path between the skin and the salivary gland had been established. The zoopsychologist, who endeavours to penetrate into the dog's soul, says that the dog notices this and remembers that the moment the irritation of a certain place of the skin begins, acid is introduced into its mouth. Consequently, when only a stimulation of the skin is applied, it imagines that the acid is entering its mouth and reacts to it by a flow of saliva, etc. Let it be so. I proceed now to another experiment. A reflex elaborated by us, repeated itself every time with absolute precision. I put now into action the mechanical apparatus and obtain as usual a complete motor and secretory reaction; however, this time I do not introduce the acid. After an interval of one or two minutes, I repeat the experiment. Now the action is already less; the motor reaction is not so pronounced and the salivation not so abundant. Again I refrain from introducing the acid, and after two or three minutes repeat the mechanical stimulation. The reaction is now still less. After four or five repetitions there is no reaction at all; nor is there any movement or salivary secretion. This is a clear and absolutely exact fact. And here is how it is interpreted by the physiologist and the zoopsychologist. I say that the phenomenon of inhibition, already well known to us, sets in. I base this affirmation on the fact that if I interrupted the experiment and made an interval of, say, two hours, the mechanical stimulation would again produce its action on the salivary gland. As a physiologist I understand this quite clearly. It is an established fact that with the lapse of time all

processes in the nervous system become effaced, if the cause which provokes them ceases to act. The zoopsychologist, in his turn, has no difficulty in providing an explanation; the dog, he says, notices that now, upon mechanical stimulation, no acid is introduced into its mouth, and therefore after four or five of these skin stimulations it ceases to react. There is no difference so far: you can agree with one as well as with the other. But let us introduce a new complication into our experiment. When the zoopsychologist and the physiologist contend with each other for the appropriateness, correctness of their explanations, it is necessary to formulate the requirements with which the explanations must comply. These requirements are well known. One of them is that each explanation must cover all that occurs physically. The facts must be explained from a single point of view. That is the first requirement, and the second is even more obligatory; it consists in being able, on the basis of the given interpretation, to predict the explained phenomena. He who is able to do so, is right compared with the one who cannot. And inability to predict signifies the bankruptcy of the latter.

I complicate my experiment in this way. Our reflex has been elaborated at several points on the dog's skin, let us suppose, at three points. The mechanical stimulation of each of these places produces the same acid reaction measured by a definite amount of saliva. This measurement is the simplest, since measurement of the motor reaction would be more difficult. The motor and salivary reactions go together and are parallel. They are the components of a single complex reflex. So, we have formed a number of cutaneous reflexes. They are all equal, act with absolute precision and give the same number of divisions of the graduated tube used in measuring the secretion of saliva, for example, thirty divisions for one half-minute stimulation. I stimulate the point on the foreleg in the way I have just mentioned, i.e., without accompanying it by the introduction of acid, so that after the fifth or sixth application

the mechanical stimulation does not produce any effect whatever. Thus, in the terminology of the physiologists, a complete inhibition of the reflex has taken place. When this has been obtained on the foreleg, I bring another mechanical apparatus into action and begin to stimulate a spot on the hind-leg. The following phenomena are then observed. If, immediately after discontinuing the foreleg stimulation which produced a zero effect, I bring the mechanical apparatus on the thigh into action, so that there is no interval between the end of the first stimulation and the beginning of the second, I obtain a complete effect at the new place equalling thirty divisions, and the dog behaves as if the stimulation were applied for the first time. The flow of saliva is abundant and there is a motor reaction—the dog tries to eject the non-existent acid from its mouth with the help of the tongue, in short, it acts accordingly. Then, when I bring the effect of stimulation on the foreleg again to zero (by repeating the mechanical stimulation without the acid), and when I stimulate the point on the hind-leg not after a zero interval but after five seconds, I obtain not thirty divisions from the new place, but only twenty. Thus the reflex has diminished. I then prolong the interval to fifteen seconds and obtain a slight effect from the new place, equalling only five divisions. Finally, if I stimulate after twenty seconds, there is no effect at all. Continuing, I extend the interval to thirty seconds; the effect at the new point reappears. With an interval of fifty seconds the effect becomes greater, reaching twenty-five divisions, and after an interval of sixty seconds it is again complete. If, after obtaining the zero result, we repeat the stimulation at the same point, on the shoulder, with intervals of five, ten, fifteen minutes, the effect will always be zero. I am not sure whether I have made this quite clear to you. What does this signify?

I invite the zoopsychologists to give their explanation of these facts. I must tell you that on more than one occasion I have gathered intelligent people, men with profound

knowledge in the domain of natural sciences, doctors, etc., placed before them the facts to which I have just called your attention, and requested an explanation of the phenomena. Most of the naive zoopsychologists attempted to give their explanations, each in his own way but at variance with all others. In general, the result was deplorable. All possible and extremely diverse interpretations were examined, but they could in no way be reconciled. Why is it that on the shoulder, when a zero effect has been obtained, the stimulating apparatus no longer produces any action, while at the other point, we obtain now a complete effect, and now no effect at all, strictly depending on the intervals between the stimulations.

And I have come here in the hope of getting an answer to this question from the point of view of the zoopsychologist. I shall now tell you about our viewpoint. Our explanation is of a purely physiological, purely material and spatial character. It is obvious that in our case the skin represents a projection of the cerebral mass, and that different points of the skin are a projection of corresponding points of the brain. When at a definite point of the brain we evoke a certain nervous process by stimulating the corresponding part of the skin on the shoulder, this process does not remain in the same place, but shows a certain movement. First it irradiates over the cerebral mass, and then returns to the point of origin and concentrates there. Naturally, each movement requires a certain amount of time. When, after elaborating an inhibition at the point of the brain corresponding to the shoulder, I immediately try to stimulate another place (the thigh), the inhibition has not yet spread to that place. It takes twenty seconds to get there; that is why in twenty seconds, and not earlier, the thigh also becomes fully inhibited. The concentration requires forty seconds, and therefore one minute after the end of the zero stimulation on the shoulder, we already have a complete restoration of the reflex at the second place (the thigh); but in the initial place (the shoulder) the reflex is absent even

after five, ten or fifteen minutes. Such is my interpretation, the interpretation of a physiologist. I have not had any difficulty in explaining this phenomenon. For me it fully coincides with other facts in the physiology of the movements of a nervous process. Now, gentlemen, let us verify the correctness of this explanation. I have the means for doing so. If we actually have a movement, then we can predict the intensity of the effect at all the intermediate points, on the basis of the fact that this movement occurs in two opposite directions. I shall take only one intermediate point. What is to be expected there? Being nearer to the point where I produce the inhibition, it will become inhibited earlier than the other points. Consequently, the zero effect will appear at this point sooner and persist longer, until the inhibition spreads and then returns. At this point the return to normal excitability will require more time. Precisely this occurred in the actual experiment. At the given intermediate point after a zero interval there were not thirty but twenty divisions. Then the zero effect appeared after ten seconds, when the complete inhibition reached this place; it persisted for a long time, while the inhibition was irradiating and then returning to the initial point. It is understandable that while the normal excitability on the thigh was re-established after one minute, here it reappeared only after two minutes. This is one of the most striking phenomena ever observed by me in the laboratory. A definite process is taking place in the depth of the cerebral mass, and its movement can be predicted with mathematical precision. So there you have, gentlemen, our complication of the experiment, and our attitude, the attitude of physiologists, in relation to it. I do not know what answer I shall get from the zoopsychologists, what attitude they will take in regard to these facts, but answer they must. If, however, they refuse an explanation, I shall be justified in saying that their point of view is in general unscientific and of no use for creative research.

REPLY OF A PHYSIOLOGIST TO PSYCHOLOGISTS[109]

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The article by Edwin R. Guthrie "Conditioning as a Principle of Learning,"* it seems to me, is of special interest because of its fundamental tendency—in my opinion fully justified—of basing the phenomena of psychical activity on physiological facts, i.e., of uniting, identifying the physiological with the psychological, the subjective with the objective, which, I am convinced, is the most important scientific task of our time. The author analyses the problem of learning from the general aspect and characterizes this process by enumerating its fundamental features; in this he utilizes without distinction both the material of psychologists and the physiological facts obtained by us on animals by the method of conditioned reflexes. Thus the psychologist and the physiologist marched side by side. But beyond this point profound differences arose between them. The psychologist regards conditioning as the principle of learning; he considers that this principle is not subject to any further decomposition, i.e., does not require further investigation, and he endeavours, therefore, to base everything on it, to represent all the separate sides of learning as one and the same process. For this purpose he takes a physiological fact and in a definite way attaches to it certain significance in

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* *Psychological Review*, Vol. 37, No. 5, 1930. (*Note by I. P. Pavlov.*)

interpreting particular facts relating to the process of learning, without seeking actual confirmation of this significance. Hence, the physiologist tends, willy-nilly, to think that the psychologist, who only recently departed from the philosopher, has not yet fully renounced his inclination for the philosophical method of deduction, for pure logical activity which does not verify every step of thought by agreement with reality. The way of the physiologist is the reverse of this. At every moment of his investigation he endeavours to analyse the phenomena separately and concretely, to determine as much as possible the conditions for their existence, without relying on mere deductions or mere hypotheses. This I shall try to prove on the basis of certain points in which the author opposes me.

Although conditioning, association by simultaneity, conditioned reflexes serve as the factual point of departure in our research, they are, nevertheless, subjected to further analysis by us. We have before us the following important question: what elementary properties of the brain mass underlie this fact? This question has not yet been finally solved by us, but certain data for its solution are afforded by the following experiments. With our experimental animal (the dog) it was observed that when the external agent, which we wish to use as a conditioned stimulus, is applied after the beginning of the unconditioned stimulus, we get a conditioned reflex (according to the latest and most precise experiments carried out by Dr. N. V. Vinogradov), but it is insignificant and temporary, and invariably disappears if the same procedure is prolonged. A stable and durable conditioned reflex, as we have long known, can be obtained only when the external agent constantly precedes the unconditioned stimulus. Thus the first procedure has a double effect: at first it contributes temporarily to the formation of the conditioned reflex, and then abolishes it. This latter effect of the unconditioned stimulus is clearly manifest in the following experiment. A conditioned stimulus, well elaborated by means of the second, usual procedure—if

afterwards it is systematically applied following the onset of the unconditioned stimulus, or is covered by it, in our laboratory terminology—gradually loses its positive action (especially when it belongs to the category of weak, conditioned stimuli) and finally is even transformed into an inhibitory stimulus. Obviously in this case the mechanism of negative induction (according to our old terminology, the mechanism of external inhibition) gradually prevails, i.e., the corresponding cell of the conditioned stimulus is inhibited, reaches a state of inhibition under the influence of repeated concentration on the part of the unconditioned stimulus—and the conditioned stimulus thus meets in its cell a constant state of inhibition. And it is this which makes the conditioned agent inhibitory, i.e., when applied alone it now evokes in its cortical cell not an excitatory but an inhibitory process. Consequently, during the usual procedure of elaboration of a stable conditioned reflex, the passage of a wave of excitation from the corresponding cortical cell to the centre of concentration of the unconditioned stimulus represents precisely the principal condition for the fixation of the path from one point to another, for a more or less constant union of the two nervous centres.

Let us pass now to other particularities of the conditioned activity where the author proposes his own uniform interpretation of the phenomena instead of our diversified analysis of concrete facts. The delayed, retarded conditioned effect, according to our experiments, is based on special inhibition of early phases of the conditioned stimulus, which do not coincide closely with the time of the appearance of the unconditioned stimulus. The author alleges for some reason that we attribute this to "mysterious latencies" in the nervous system, and gives his own interpretation of the facts. He admits that when, for example, the sound of a bell plays the role of a conditioned stimulus, the animal responds with a reaction of strenuous listening, with a complex motor act, and the centripetal impulses of this act are, strictly speaking, the real stimulators of the conditioned

effect, in our case of the conditioned alimentary reflex—the salivary secretion.

According to the author, "when the salivary glands begin to secrete, the accompanying stimuli are not furnished by the bell, but by these responses to the bell. The direct response to the bell is probably over in a small fraction of a second." And further he states: "The apparent separation in time of a conditioning stimulus and its response is then quite possibly an illusion." The author even says that "Pavlov tends to forget in his explanation of the delay" the existence of the above-mentioned centripetal impulses from the motor apparatus. On page 312 of my "Lectures on the Work of the Cerebral Hemispheres"* one can see that not only do I take into account the centripetal impulses for the skeletal musculature, but I regard it as being more than probable that they exist even for all the tissues, to say nothing of the separate organs. In my view, the entire organism with all of its components are able to report about themselves to the cerebral hemispheres.[110] Consequently, this is not the matter of an omission on my part; the matter is that actually we have not the slightest grounds for interpreting the fact in the way the author does.

First of all, if we agree with him that it is not the bell, but the centripetal impulses from the motor act of strenuous listening that is the actual stimulus for the conditioned effect, then why does the effect not manifest itself at once, but is retarded (in the case of a delayed reflex) and, besides, in accordance with the length of the interval between the beginning of the stimulus and the beginning of the unconditioned reflex? For, when the unconditioned stimulus is delayed for a shorter time—only a few seconds—after the beginning of the conditioned stimulus, the effect, which, according to the author is caused by the centripetal impulses from the motor act of listening, also manifests itself very quickly, namely, in two or three seconds. How, then, does

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* Second edition. (*Note by I. P. Pavlov.*)

he explain the duration of the delay? And why, when the unconditioned stimulus is separated from the beginning of the conditioned by an interval of a few minutes, do the stimuli admitted by the author (namely, the centripetal impulses of movement) act after a lapse of minutes?

Besides, there are no grounds whatever for admitting a constant action of the stimuli of which the author speaks. The listening reaction, like the general orienting or investigatory reflex, as I have termed it, evoked by any new fluctuation in the habitual surroundings of the animal, usually exists only during the first brief period of application of the new recurring stimuli; when a conditioned reflex is formed, with a more or less short interval between the conditioned and unconditioned stimuli, it is quickly superseded by a special motor reaction peculiar to the given unconditioned stimulus. Subsequently, only the conditioned motor effect is permanently manifest, and there is no trace of an orienting reaction. The conditioned stimulus now becomes a pure substitute for the unconditioned stimulus. In the case of a conditioned alimentary reflex the animal may lick the flashing electric bulb, or attempt to take the air into its mouth, or try to eat the sound itself; in doing this the animal licks its lips and grinds its teeth, as if dealing with real food. The same thing takes place in the case of an elaborated delayed reflex. The animal remains wholly indifferent and quiet during the first period of action of the conditioned stimulus; not infrequently, immediately after the beginning of the stimulus, it even falls into a state of drowsiness and sometimes into a state of profound sleep (accompanied by relaxation of the musculature and snoring). With the beginning of the second period of the conditioned stimulation, just a little before the addition of the unconditioned stimulus, this state is replaced, sometimes impetuously, by a clearly pronounced corresponding conditioned motor reaction. In both cases it is only during the general somnolence of the animal in the course of the experiment that the orienting reaction now and then

reappears at the first moment of the action of the stimulus.

And finally, the retardation in question is actually not the result of a "mysterious latency," but of the interference of a special, induced inhibition, which is well known to us and has been studied in detail in its various manifestations. The matter is quite clear. Although the considerably prolonged conditioned external stimulus remains one and the same, for the central nervous system and especially, one must suppose, for the cerebral hemispheres, it is obviously different at different periods of its course. This is particularly manifest with olfactory stimuli, which are sensed by us at first keenly and then rapidly become weaker and weaker, although objectively they remain constant. Apparently the state of the stimulated cortical cell under the influence of an external stimulus undergoes successive changes, and in the case of a delayed reflex only that state of the cell which closely precedes the addition of the unconditioned reflex plays the role of a signal conditioned stimulus. Exactly the same thing occurs when from different intensities of one and the same external stimulus we elaborate different conditioned stimuli, now positive, now negative, and now related to different unconditioned stimuli. The fact of delay under consideration represents an obviously interesting case of special adaptation, to ensure that the conditioned reflex does not appear too early, and that energy should not be expended beyond the necessary measure. That this explanation conforms to reality is proved by facts. Above all, this is clear from the procedure of formation of a delayed reflex. If the conditioned reflex is first elaborated with an interval of a few seconds between the beginning of the conditioned and of the unconditioned stimuli and the interval is then suddenly increased to a few minutes, the conditioned effect, which earlier manifested itself rapidly, gradually diminishes and soon fully disappears. And then, when the experiment is considerably prolonged, there comes a period of absence of any conditioned

effect. Only afterwards does the conditioned effect reappear, at first just before the moment of the addition of the unconditioned stimulus; then it grows gradually and begins to manifest itself somewhat earlier.

That the first period of the delayed reflex is actually a period of inhibition, is proved by a number of facts. In the first place, the inhibition of the delayed reflex can easily be summated. In the second place, a successive inhibition can be observed from this reflex. And finally, the drowsy and sleeping state which arises in some animals in the first part of the delayed reflex, is a striking manifestation of the inhibitory state.

The next phenomenon—the extinction of the conditioned reflex—is also discussed by the author without any consideration of the factual details of our investigation; he has in view again the factor which he himself conjectured but did not define more precisely, and at the same time he attributes to me, in addition to the previously mentioned "inclination to forget," also a tendency "to conceal something from myself."

First of all the author, contrary to our affirmation, assumes that it is not the short duration of the interval between repetitions of the non-reinforced conditioned stimuli that contributes to the extinction of the conditioned reflexes, but the number of these repetitions. However, this is absolutely wrong. A non-reinforced conditioned stimulus without any repetitions, but merely prolonged for a period of three to six minutes, invariably ends in extinction to absolute zero, or as we term it, continuous extinction, in contrast to an intermittent one. Further, the author with the same arbitrariness supposes that extinction is not a constant fact, but an exception to the rule of frequency. And here, too, he is wrong. Extinction is one of the most constant facts of the physiology of conditioned reflexes. Having made both these conclusions contrary to reality, the author, so to speak, clears the field for himself and imagines the existence of other agents, which he does not define more precisely

and which, together with the basic unconditioned stimulus, take part in the formation of the conditioned effect. It is probable that here, too, movements of the animal are implied, since mention is made of continuous and diverse movements of the animal in the course of the experiment. Thus, according to the author, the sum of the agents which determine the conditioned reflex constantly fluctuates, now increasing, now diminishing. When the number of these agents decreases and the conditioned reflex is absent or becomes weakened, then other, also unknown, agents become inhibitory, or what is actually the same, they become stimulators of other responses.

The author explains the interference of extraneous stimuli with extinction by asserting that these stimuli "disorganize posture and orientation," which appeared as inhibitors of the conditioned reflex at the stage of extinction and thus temporarily restore the reflex that is becoming extinguished.

The author does not consider it necessary to point out, even hypothetically, precisely which stimuli, along with the unconditioned one, support the conditioned reflex and which other stimuli that are also present inhibit this effect. When the author explains in his own way the interference of extraneous stimuli with the extinction, why does he not show how the extraneous stimuli, which remove the action of the agents inhibiting the conditioned effect, do not remove also the action of those stimuli supporting the conditioned response? After all, these are quite different stimuli.

Thus, the author has introduced, without any factual confirmation of their real value, a multitude of undefined, unknown stimulating agents.

It is to be assumed that the author has in mind just the same kinesthetic stimuli, but originating in different muscles. Of course, there are many skeletal muscles, and their movements form an almost infinite number of combinations, constantly sending special centripetal impulses to the central nervous system. But, in the first place, most of them proceed to the lower parts of the brain, and, in the second

place, under usual conditions they in no way make themselves known to the cerebral hemispheres, serving only for the auto-regulation and greater precision of movements, for example, such as the continuous cardiac and respiratory movements. In our experiments only those movements, which form special motor reflexes, are effective in influencing our conditioned reflexes; chief and almost exclusive among these is the orienting reflex to the fluctuations of the surrounding medium, and sometimes also the defensive reflex arising in response to an accidental nocuous action on the animal during its movements in the experimental stand (a blow, some kind of pinch, etc.).

If, as the author assumes, the centripetal impulses from all the movements which we effect really reached the cerebral hemispheres to any considerable degree, then, by their very quantity alone, they would be a tremendous obstacle to the relations of the cortex with the external world and would render almost impossible this most important cortical function. Do the movements which indispensably arise when we talk, read, write, and in general when we think, disturb us to any extent? Are these actions ideally performed only when we are in a state of absolute immobility?

The constant fact of extinction is not the result of chance movements of the animal which are reflected in the work of the cerebral hemispheres; it is a law-governed manifestation of the fundamental properties of the cortical cells, as the most reactive cells in the organism, when they work for a more or less considerable length of time—even generally for a short period of time—without being accompanied by the fundamental inborn reflexes. The chief physiological function of the excitation of these cells is to serve as signals, to replace the special stimuli of the latter reflexes. Being the most reactive cells, they rapidly become exhausted, and from an active state pass into a state of inhibition, which, in all probability, not merely contributes to their rest, but accelerates their recovery. But when the activity of these cells is accompanied by unconditioned

stimuli, the latter, as noted at the beginning of the article, immediately and, so to speak, in a preventive way inhibit them and thereby assist in the recovery.

That extinction is actually inhibition is proved both by its successive inhibitory action upon other positive conditioned reflexes and by the transition to a drowsy and sleeping state which, undoubtedly, is inhibition.

As for the remaining two points, where the author, instead of our explanation, offers a similar interpretation of his own, I can be more brief. With regard to the fact of the gradual intensification of the conditioned effect in the course of its formation, it must be pointed out that in this case it is the gradual elimination of extraneous stimuli that hinders the formation of the reflex, and not, as the author assumes, their ever-increasing participation in the creation of the effect. During our first experiments often from fifty to one hundred or more repetitions were needed to elaborate a complete conditioned reflex, while now from ten to twenty repetitions suffice, and often considerably fewer. In our present experimental conditions the first application of a new indifferent agent, which must later serve as a conditioned stimulus, results only in an orienting reflex, the motor component of which in the overwhelming majority of cases steadily and rapidly diminishes all the way to complete disappearance; thus, absolutely nothing remains of the constantly-growing sum of factors determining the conditioned effect, of which the author speaks. It is clear that the whole process consists in an ever-increasing concentration of excitation, and then probably in gradually beating a path between the points of the central nervous system which are to be connected.

Finally, as regards the independent acquisition of a conditioned effect from neighbouring stimuli, or near that to which the conditioned reflex was specially elaborated, the author again differs from us. In our view, this is nothing but an irradiation of excitation over a definite part of the cortex. The author, however, assuming that not a specific

agent appears here as a conditioned stimulus, but an orienting reflex accompanying it, again gives his interpretation of the matter alleging that all the neighbouring agents become effective thanks to one and the same orienting reflex. But this completely contradicts the real facts. The neighbouring agents in most cases produce the conditioned effect directly, without any trace of the orienting reflex. On the contrary, when the latter is present, the conditioned effect is either fully absent, or considerably weakened; it manifests itself and grows only in proportion to the disappearance of the orienting reflex.

Thus, throughout the article, the author remains true to himself and to his habit of deduction.[111] Making erroneous use of a single physiological fact, he constantly and immediately derives from the principle of conditioning all the particulars of conditioned nervous activity which he utilizes for his subject of learning, while the factual aspect of these particulars are completely disregarded by him.

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It seems to me that the second article, "Basic Neural Mechanisms in Behaviour,"* to which I pass now, in large measure treats the subject in the same way as the first. This article, by K. S. Lashley, was submitted in the form of a paper to the last International Congress of Psychology held in the United States (1929). Even though its material is almost exclusively physiological, the author's approach is exactly the same as in the preceding article. The material is sacrificed to the principal preconceived tendency to prove that "the reflex theory is now becoming an obstacle rather than a help to progress" in studying the cerebral functions; according to the author, of greater force and significance in this respect is, for example, the statements of C. Spear-

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* *Ibid.*, No. 1. (*Note by I. P. Pavlov.*)

man[112] that "intelligence is a function of some undifferentiated nervous activity," or the analogy with the tissue of sponges and hydroids, which, being crushed and sifted through bolting cloth, subsequently, when settled or centrifuged down, again assumes the shape of a mature specimen with a characteristic structure.

First of all I must say in a general way, without going into detail for the time being, that this merciless judgement against the theory of reflexes is divorced from reality, an adamant and, one may even say, incomprehensible, refusal to take reality into consideration. Does the author really venture to affirm that all my thirty years' work, fruitfully carried on together with numerous colleagues, under the guiding influence of the idea of reflexes, is nothing but a hindrance to the study of the cerebral functions? No. No one has the right to say that. We have established a series of important rules relating to the normal activity of the higher part of the brain; defined a certain number of conditions of its wakeful and sleeping states; elucidated the mechanism of normal sleep and hypnosis; experimentally produced pathological states in this part of the brain, and found the means with which to restore it to normalcy. The activity of this part, as already studied by us, found and continues to find many analogies with the phenomena of our subjective world, as is evident from acknowledgements not infrequently made by neuropathologists, educators, psychologists-empiricists, as well as from the statements of academic psychologists.

Now before the physiology of this part of the brain there arises an infinite number of urgent problems, of strictly definite tasks in the field of further experimentation, instead of the almost blind-alley state in which this physiology undoubtedly found itself in the past decades. And this is the outcome of the application of the concept of reflexes to the experiments carried out on this part of the brain.

What, exactly, is the concept of a reflex?

The theory of reflex activity is based on three fundamental principles of exact scientific investigation: in the first place, the principle of determinism, i.e., an impulse, an impetus, or a cause for every given action or effect; in the second place, the principle of analysis and synthesis, i.e., the initial decomposition of the whole into its parts or units, and the subsequent gradual re-establishment of the whole from these units or elements; and finally, the principle of structure, i.e., the disposition of the activity of force in space, the adjustment of dynamics to structure. Therefore one cannot but regard the death sentence pronounced on the theory of reflexes as a misunderstanding or as bias.

You have before you the living organism, all the way to man, which produces a series of activities, manifestations of energy. You get a direct impression, difficult to overcome, of something spontaneous. In the case of man, as an organism, this impression is so strong that it is an obvious truth to almost everyone, and any contrary assertion seems absurd. Although Leucippus of Miletus* proclaimed that there is no effect without cause and that everything arises from necessity, is it not still believed, even leaving man aside, that there are spontaneously acting forces in the animal organism? As for man, do we not still here talk about free will and is there not a conviction rooted in many minds that there is something in us which is not subject to determination? I have always met and still meet not a few educated and intelligent people who are quite incapable of conceiving how it is possible to study the whole behaviour, say, of a dog quite objectively, i.e., solely by comparing the stimuli acting on the animal with its responses to them, and, consequently, disregarding the subjective world, which is presumed to exist in it by analogy with ours. What is meant, of course, is not the temporary difficulties of research, immense as they are, but the impossibility in principle of

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* I take this indication from Professor Kannabich's book *The History of Psychiatry*. (*Note by I. P. Pavlov.*)

complete determination. It goes without saying that this same conviction is far more positive with regard to man. I should not be committing a great error in assuming that this conviction persists also among some of the psychologists, although masked by references to the *peculiar nature of psychical phenomena,* behind which, despite seemingly scientific arguments, can be detected the dualism and animism that is still immediately shared by many thinking people, to say nothing of religious believers.

The theory of reflexes is, as it has been doing ever since its inception, constantly and continuously enlarging the number of phenomena in the organism connected with the conditions that determine them, i.e., it is making clearer the integral activity of the organism. How, then, can it be an obstacle to the study of the organism in general, and the cerebral functions in particular?

Further, the organism consists of a mass of separate large parts and of billions of cellular elements producing corresponding masses of separate phenomena, which, however, are closely interconnected and ensure the integrated work of the organism. The theory of reflexes divides this general function of the organism into particular activities, connecting them both with internal and external influences, and then reunites them, one with another, thus making the activity of the organism as a whole and its interaction with the environment more and more comprehensible. How, then, has the theory of reflexes proved to be superfluous or inappropriate, or how can it be, if our knowledge of the connections between the separate parts of the organism is still insufficient, to say nothing of our incomplete comprehension of all the correlations of the organism with the surrounding medium? And all the internal, as well as external, relations in higher organisms are mostly effected by means of the nervous system.

Finally, if the chemist, when analysing and synthesizing, must, for the full understanding of the work of the molecule, imagine its invisible structure, and if the physicist, when

analysing and synthesizing, must, for obtaining a clear idea of the mechanism of the atom, also visualize its structure, how is it possible to renounce the structure of visible masses and admit some kind of contradiction between structure and dynamics? The function of connecting both the internal and external correlations in the organism is accomplished in the nervous system which represents a visible apparatus. It is in this apparatus, of course, that the dynamic phenomena, which must be related to the most delicate details of its structure, are actually developed.

The theory of reflexes began the investigation of this apparatus with the definition of special functions, pertaining naturally to its more simple, grosser parts, and determined the general tendency of the dynamic phenomena occurring in it. Here is the general and fundamental scheme of the reflex: receptor apparatus,[113] afferent nerve,[114] central station (centres) and efferent nerve[115] with its effector tissue. Then came detailed elaboration of these parts, a process that is still going on. Of course, the most complicated and extensive work concerned, and still concerns, the central station, and in the parts of the central station—the grey matter, and in the grey matter—the cerebral cortex[116] This work concerns the visible structure itself, as well as the dynamic phenomena developing in it, and, of course, does not lose sight for a single moment of the indispensable connection between structure and dynamics. In view of the difference of method in studying structure and dynamics, the investigation is naturally divided for the most part between the histologist and physiologist. No histologist-neurologist would, of course, make bold to state that the study of the structure of the nervous system, and especially of the higher part of the central nervous system, has reached completion; on the contrary, he will admit that the structure of this part still remains highly confused and obscure. Has not the cyto-architectonics[117] of the cerebral cortex revealed to us quite recently its extreme complexity and diversity? And are not all the numerous variations in the

organization of different parts of the cortex without a definite dynamic importance? If the histologist is able to some extent to analyse them, how can the physiologist fully trace the movement of the dynamic phenomena along this inconceivable network? The physiologist, basing himself on the reflex scheme, never regarded the investigation of the central station as having been elaborated to any considerable extent, even in the simplest structures of these centres; he has always adhered to and has always been guided by the fundamental concept of the passage, the transmission of the dynamic process from the afferent to the efferent conductor. As for the higher centres, besides relating the functions to the details of structure as much as possible, he, of sheer necessity, concentrates his attention and work for the time being mainly on the dynamics, on the general functional properties of the brain mass. This has been done, and was continued until recent times, mostly by the Sherrington, Verworn and Magnus schools, as well as by other individual authors, on the lower parts of the brain; as for the higher part, this is being done predominantly and in the most systematic way by me and my colleagues in applying the conditioned reflex variation of the general theory of reflexes.

With regard to the cerebral cortex, the first reliable data relating to the detailed connection between its activity and structure, were obtained at the beginning of the glorious epoch of the seventies of the last century. While the existence of a special motor region in the cortex was then established and subsequently confirmed by successive investigators, the very exact and restricted localization of the sense organs in the cortex, as originally described, met with objections both on the part of physiologists and neuropathologists. To some extent these objections shook the theory of localization in the cortex. For a long time the situation remained uncertain, because the physiologist did not have purely physiological knowledge of the normal activity of the cortex; the use of psychological concepts, when psychology has not yet created a natural and generally ac-

cepted system of its phenomena, could not, of course, be conducive to the further study of the problem of localization. The situation radically changed when, thanks to the theory of conditioned reflexes, the physiologist was at last provided with the possibility of seeing the special, and at the same time purely physiological, activity of the cerebral hemispheres and thus could clearly distinguish the physiological activity of the cortex from the activity of the nearest subcortex, and the lower parts of the brain in general, in the shape of conditioned and unconditioned reflexes. Then all the separate facts could be definitely and strictly systematized, and the main principle of the structure of the cerebral hemispheres came clearly to the fore. From the seventies on, the special regions which had been located in the cortex as centres for the principal external receptors, remained the seat of higher synthesis and analysis of corresponding stimuli; however, along with these regions one had to admit the existence of representatives of the same receptors, dispersed, perhaps, over the entire cortex or at least over a considerable part of it, but capable only of simpler and quite elementary synthesis and analysis. A dog that had been deprived of the occipital lobes could not distinguish one object from another, but did distinguish degrees of light and simplified forms. A dog without temporal lobes was unable to discriminate complex sounds, such as its own name, etc., but was still capable of exact differentiation of separate sounds, for example, of one tone from another. What convincing proof of the fundamental importance of specialized structure!

Not without interest is the following experiment carried out by Dr. Elliason, and described in my "Lectures on the Work of the Cerebral Hemispheres"; it provides more detailed indications with regard to the functional significance of the structural properties of the special cortical regions. An acoustic complex of three tones of a harmonium—two extreme and one intermediate—covering more than three and a half octaves, and applied simultaneously, was used

for the elaboration of a complex conditioned alimentary stimulus which evoked the secretion of a certain amount of saliva indicating the degree of intensity of the alimentary reflex. Subsequently, when separate components of the acoustic complex were tested, they also produced salivation, but to a lesser extent than the whole complex; the intermediate tones between these also caused a secretion of saliva, which was still less abundant. Then a bilateral extirpation of the anterior temporal lobes (gg. sylviaticus and ectosylvius with the anterior part of g. compositus posterior) was performed. The following was observed. When all the conditioned reflexes (to stimuli from other analysers) were restored after the operation, as well as the conditioned reflex to the chord (the latter reflex even before some of the others), the reflexes to isolated component tones of the chord were tested anew. The high tone and the neighbouring tones lost their effect, while the middle and low tones, as well as the intermediate tones, retained it; the effect of the low tone even increased, becoming equal to that produced by the whole chord. But when the high tone was accompanied separately by feeding, it soon (at the fourth essay) became again a conditioned alimentary stimulus and acquired a considerable effect—not a weaker, but a stronger one than previously. A number of exact conclusions can be drawn from this experiment. In the first place, that separate elements of the receptor acoustic apparatus are represented at different points of the special auditory region of the cortex; in the second place, that the complex stimuli use precisely this region; and, in the third place, that representatives of the same elements of the auditory apparatus, dispersed over a considerable part of the cortex, take no positive part in these complex stimuli.

When one sees, as I have seen, with the conditioned reflexes in hand, that a dog, with the posterior, greater part of both hemispheres removed, can orient itself with extreme precision by means of its cutaneous and olfactory receptors, losing only *complex* visual and auditory relations with the

surrounding world, i.e., becoming unable to distinguish complex visual and auditory stimuli; that a dog deprived of the upper halves of both hemispheres, while fully retaining complex relations (auditory) with the surroundings, loses—in an astonishingly isolated manner—only the ability to orient itself amid the solid bodies met in the surrounding medium; and that, finally, a dog with extirpated anterior (minor) halves of both hemispheres, is, to all appearances, a fully disabled animal, i.e., deprived chiefly of proper locomotion, of the normal use of its skeletal movements, and yet reveals by means of another indicator, namely, the salivary gland, its complex nervous activity. When one sees all this how is it possible not to be impressed first of all with the paramount role played by the structure of the cerebral hemispheres in the fundamental task of the organism—to ensure proper orientation in the environment, proper equilibration with it? How, after this, is it possible to doubt the further significance of the more detailed features of the structure?

Were we to adopt the viewpoint of our author, described below in detail, we should have to advise the brain histologists to abandon their work as unnecessary and useless. Who would venture to draw such a conclusion? Otherwise, all the structural details, which are disclosed, must sooner or later find their own functional significance. Therefore, along with the further and deeper histological study of the cortical mass, it is necessary to carry on a purely and strictly physiological investigation of the activity of the cerebral hemispheres and of the adjacent part of the brain, so that the two elements—structure and function—could be gradually connected one with the other.

And this is done by the theory of reflexes.

Physiology long ago firmly established the permanent connection of definite internal and external stimuli with definite activities of the organism expressed in the form of reflexes. The theory of conditioned reflexes undoubtedly established in physiology the fact of temporary connection

between diverse (and not merely definite) external and internal stimuli with certain units of activity of the organism, i.e., along with conduction of nervous processes in the higher centres, it also explicitly stated the possibility of their connection and disconnection. But this addition, of course, did not lead to any essential changes in the concept of a reflex. The connection between a definite stimulation and a certain unit of activity of the organism remains; however, it is manifested exclusively in the presence of a definite condition. That is why we distinguish these reflexes from inborn reflexes and term them "conditioned," in contradistinction to the older ones which have been termed "unconditioned." Thanks to this, the investigation of conditioned reflexes is based on the same three principles of the reflex theory: determinism, gradual and consecutive analysis and synthesis, and the structural principle. For us, the effect is always connected with the cause, the whole, in ever-increasing measure, is divided in parts and then synthesized anew, and the dynamics remain connected with structure, inasmuch, of course, as this is granted by the data of present-day anatomical investigation. Thus, one can say, boundless vistas are opened up for research into the dynamics of the higher part of the brain, i.e., of the cerebral hemispheres and of the adjacent subcortex with its most complex fundamental unconditioned reflexes.

We consecutively study the fundamental properties of the cortical mass, define the essential activities of the cerebral hemispheres and elucidate the connections and interrelations of the cerebral hemispheres and the adjacent subcortex.

The basic processes of cortical activity are excitation and inhibition, their movement in the form of irradiation and concentration, and their reciprocal induction. The special activity of the cerebral hemispheres is expressed in a continuous analysis and synthesis of stimuli coming both from the external environment (for the most part) and from within the organism; thereafter the stimuli are directed to

the lower centres beginning with the adjacent subcortex and ending with the cells of the anterior horns of the spinal cord.

Thus, under the influence of the cortex, the entire activity of the organism becomes more and more exactly and delicately correlated or equilibrated with the environment. On the other hand, the adjacent subcortex sends a powerful stream of impulses from its centres to the cortex which maintains the tonus of the latter. In the final analysis, the centre of gravity of research into the higher part of the brain is now being transferred to investigation of the dynamic phenomena taking place in the cerebral hemispheres and in the adjacent subcortex.

As stated above, the work of the cortex essentially consists in analysis and synthesis of the stimuli entering it. The variety and number of these stimuli is truly innumerable, even in an animal like the dog. The best way to express the number and variety of stimuli would be to say that all the stages through which the states of separate cortical cells and of their diverse combinations pass, represent individual stimuli. By means of the cortex it is possible to form special stimuli from all gradations and variations of the excitatory, as well as of the inhibitory, processes, both in individual cells and in their various combinations. Stimuli formed from different intensities of one and the same stimulation, from the relationship of stimulations, etc., may serve as an example of the first case, and different conditioned hypnogenous stimuli—as an example of the second.

These countless states of the cells are formed not only under the influence of existing stimuli and manifest themselves not only during the action of external stimulations, but remain also in their absence, in the form of a system of different intermittent, more or less stable degrees of excitation and inhibition. Here is an illustration. A series of positive stimuli of different intensities, and of negative stimuli, are applied by us day by day for a certain period, in one and the same sequence and with the same intervals

between them; in this way we obtain a system of corresponding effects. If in the course of the experiment we repeat only one of the positive stimuli, observing the same intervals, it will reproduce exactly the same fluctuations of the effect which were caused by all the successive stimuli taken together in the previous experiments, i.e., the same system of states of cortical excitation and inhibition will recur.

Of course, one cannot claim at the moment the establishment of any far-reaching conformity between the dynamic phenomena and the details of structure, but this conformity must necessarily be admitted, for the structure of the cortex is highly variegated throughout, and we are already convinced that certain degrees of synthesis and analysis of stimuli are accessible only to definite parts of the cortex, being inaccessible to others. This is also firmly established by the following fact. When a series of different acoustic stimuli (a tone, noise, beats of the metronome, a gurgling sound, etc.), or mechanical stimulations at different spots of the skin, are developed into conditioned stimuli, we can render one of the stimulated points pathological and invalid, while the other will be quite normal. We obtain this result not in a mechanical, but in a functional way, by bringing the point of stimulation into a difficult state either by an excessively intense stimulation or by a drastic collision at that point of the excitatory and inhibitory processes. And how can this be explained except by saying that the excessive work imposed by us on the given minute detail of structure, resulted in its destruction, just as rough handling spoils and destroys a very delicate instrument? How delicate and specialized must these details be when the points of application of other auditory and mechanical stimuli remain fully preserved and intact. Such isolated destruction can hardly ever be obtained mechanically or chemically. After this there can be no doubt that if at present, following mechanical destruction of the cortex, we sometimes do not observe any change in the animal's

behaviour, this is due to the self-evident fact that we have not yet decomposed the animal's behaviour into all its elements, the number of which must be enormous. And for this reason, the disappearance of some of them naturally escapes our attention.

I have taken the liberty of dwelling at length on our data because I wanted, in the first place, to make further use of them in criticizing the experiments and respective conclusions of Lashley, and, in the second place, to show once more how fruitful at the present time is the investigation of the cerebral hemispheres based on the theory of reflexes with all its principles.

What, then, are the objections raised by Lashley against the theory of reflexes? What are the arguments with which he tries to smash this theory?* First of all it is absolutely clear that he conceives it in a peculiar way. Arbitrarily, disregarding physiology, he approaches it exclusively from the structural point of view, without a single word about its other principles. It is generally accepted that the idea of the reflex dates from Descartes. But what was known in Descartes' time about the detailed structure of the central nervous system, especially in connection with its activity? Indeed, it was only in the beginning of the 19th century that a physiologico-anatomical distinction was made between the sensory and motor nerves. It is obvious that the idea of determinism formed the essence of Descartes' notion of a reflex, and originated his concept of the animal organism as a machine. All later physiologists interpreted the reflex in a similar way; they related the individual activities of the organism to definite stimuli, gradually specifying the elements of nervous structure in the form of different afferent

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* Since the monograph by K. S. Lashley entitled "Brain Mechanisms and Intelligence," published simultaneously with the abovementioned address, gives a fuller exposition of the author's experimental material, I shall, henceforth, refer to the address and monograph without any distinction, citing facts, conclusions and extracts. (*Note by I. P. Pavlov.*)

and efferent nerves and in the form of special paths and points (centres) of the central nervous system, and finally, collated the typical features of the dynamics of this system.

The main factual grounds which serve Lashley for his conclusion concerning the harmfulness of the reflex theory at the present time, and for recommending a new concept of cerebral activity, are drawn chiefly from the author's own experimental material. This material consists mainly of experiments carried out on white rats which learn to find the shortest possible way to a compartment containing food in a more or less complicated maze. These experiments showed that the learning became more difficult almost exactly in proportion to the degree of preliminary destruction of the cerebral hemispheres, and, besides, irrespective of the parts subjected to destruction; in other words, the result was determined exclusively by the mass of hemispheres which remained intact. After additional experimentation the author reached the conclusion that "specific cortical areas, and association or projection tracts, seem unessential to the performance of such functions which rather depend upon the total mass of normal tissue." Thus, the author draws an original, but actually quite inconceivable, conclusion that the most complex activities of the apparatus are effected without the participation of its special parts and chief connections, or, in other words, that the whole apparatus functions somehow or other independently of its component parts.

And so the main question is: why is the accomplishment of the maze task regularly retarded, depending solely on the degree of destruction of the hemispheres, but irrespective of the location of destruction? In this connection it is to be regretted that the author did not keep in mind the theory of reflexes with its first principle of determinism. Otherwise, the first question he would have put to himself, when analysing his experimental methods, would be the following: how could the rat solve the maze problem in general? After all, it could not do it without some kind of

directing impulse, without some kind of signal. Were we to take the contrary view, no matter how difficult this is, we would be obliged to show that the task can be actually accomplished without the help of any stimuli, i.e., it would be necessary first to destroy in the rat *all receptors at once* But who has ever done this and how can it be done? If, however, as may be naturally assumed, signals, or certain stimuli, are inevitably required for the solution of the task, then the destruction of individual receptors or of their definite combinations is obviously insufficient. It is possible that all or almost all the receptors participate in the response, superseding one another separately or in certain combinations. And with rats, in view of the well-known conditions of their life, this invariably is the case. It is not difficult to assume that in accomplishing the maze task the rat makes equal use of olfactory, auditory, visual, cutaneous, and kinesthetic stimuli. And since special regions of these receptors are located in different places of the cerebral hemispheres, and individual representatives of their elements are possibly dispersed throughout the entire mass of the hemispheres, the possibility of solving the task always remains, no matter how much of the hemispheres has been extirpated; but, naturally, the less cortical tissue that remains intact the more difficult is the solution. But if one accepts the view that in the given case the rat uses only one receptor or a few of them simultaneously, then it is necessary to prove this by special experiments that would leave no room for doubt, i.e., by allowing each receptor to act separately or in certain combinations, and excluding all others. But, as far as I know, neither the author nor anyone else has ever performed such experiments.

It is very strange that the author completely disregards all these possibilities and does not even put the question: how does the rat overcome the mechanical obstacles, what stimuli, and what signals serve for the corresponding movements? He confines himself to experiments which involve the destruction of individual receptors separately or in cer-

tain combinations, but do not abolish the habit; he ends his analysis by stating that "the most important features of the maze habit are a generalization of direction from the specific turns of the maze and the development of some central organization by which the sense of general direction can be maintained in spite of great variations of posture and of specific direction in running." Truly, one can say, some kind of bodiless reaction!

Various incisions in the cerebral hemispheres and in the spinal cord were applied by the author as additional experiments in studying the maze reaction; their purpose was to exclude the association and projection tracts in the hemispheres and the pathways in the spinal cord. However, it should be pointed out that all these methods, as physiologists well know, are in no way decisive, but rough, approximate methods, and the more so the more complicated the structure. This applies also to the peripheral nervous system which is much more crude and simple. Physiologists know very well how difficult it is fully to isolate the organs from nervous connections with the whole organism, and often only complete removal of an organ gives full assurance in this respect. Various crossings, loops, etc., in the peripheral nervous system are familiar to physiologists. Let us recall, for example, the case of recurrent sensibility in the spinal roots and the innervation of a single muscle by fibres from different roots. How much more diverse and delicate, then, must this, so to speak, mechanical immunity be in the central nervous system with its immense connections. It seems to me that up to now this highly important principle has not been sufficiently recognized, particularly in the physiology of the nervous system, and has not been clearly and constantly formulated. Indeed, the system of the organism developed in the midst of all the surrounding conditions: thermal, electrical, bacterial and others, including mechanical conditions; it had to balance all these conditions, adapt itself to them, and prevent or limit their destructive action as much as possible. In the nervous sys-

tem, and especially in its most complex central part, which controls the entire organism and unites all its particular activities, this principle of mechanical self-protection, the principle of mechanical immunity, had to attain the highest degree of perfection, and this, actually, is mostly the case. Since at present we cannot claim complete knowledge of all the connections which are formed in the central nervous system, our experiments with incisions, sections, etc., in many cases are, in essence, only of a negative character, i.e., we do not accomplish our aim of disjunction, since the apparatus proves to be more complicated, so to speak, more auto-regulatory than we anticipated. It is, therefore, always risky to draw definite and far-reaching conclusions on the basis of such experiments.

In connection with our first question I shall touch on the problem of relative complexity of habits which the author investigated; I do so mainly for the sake of appraising his methods. The author finds that the maze habit is more complicated than the habit of distinguishing between different intensities of light. But how is this proved? Actually the reverse is the case: a habit in a highly complicated maze is formed in the course of nineteen trials, while the habit of distinguishing the brightness of light is formed in 135 trials, i.e.., the maze habit is elaborated seven times easier. If a comparison is made with the simplest of the three mazes used by the author, the difference in the degree of difficulty amounts almost to thirty times. Despite this, the author arrives at the conclusion that the maze habit is more complex. He backs his conclusion with various explanations; however, in order to be convincing he should have determined exactly, that is, quantitatively, the value of the facts suggested in his explanations, so that, taken together, they should not only cover the actual difference, but transform the result into its opposite.

In a situation of this kind I would not venture to say what is complex and what is simple. Let us examine the essence of the question. In the movements of the animal in

the maze and in the box with different intensities of illumination account is taken only of its turns to the right or to the left, but not the whole act of locomotion. In both cases signals or special stimuli are necessary to effect the turns. And in both cases they actually exist. But beyond this there is a difference. There are several turns in the maze, in the box only one. Consequently, in this respect the maze is more difficult. But that is not the only difference. In the maze the signs for the turns differ almost exclusively in quality; for example, in the act of turning the animal comes into contact with the openings of the partition now at the right side, now at the left side of the body; in making the turns the muscles of the right and left sides work alternately. The same is true in respect of the visual and auditory signals. In the box it is a matter of quantitative difference. These differences must somehow be equilibrated. And besides, of course, the life experience of the rat is bound to play a part, i.e., its greater or lesser preliminary acquaintance with one or another task, as the author rightly points out. But it is also impossible to overlook the fact that in the most complicated maze the accomplishment of the task is greatly facilitated by definite rhythm, by alternation of the turn now to the right, now to the left. On the other hand, inculcation of ability to distinguish between different intensities of light is bound to be considerably influenced by the fact that two impulses contribute to it: food and nocuous stimulation (pain), whereas in the maze the habit is determined only by food. This, of course, complicates the conditions of learning. And still another question arises: do the two impulses contribute to the formation of the habit, or, on the contrary, impede it? Besides, we have stated above that the development of a system of effects is a very easy and persistent phenomenon in nervous activity. So that in both methods, in the maze and in the box, we have different conditions, with the result that an exact comparison of the difficulty of the task becomes almost impossible. All this, plus the above-indi-

cated vague character of the signals in the maze, makes the entire method of the author highly problematic.

That our author is more inclined to theorize and to generalize than to perfect and vary his experiments (an indispensable requirement in biological experimentation), can be seen from the following investigations carried out by him in relation to the same problem.* In one of these researches he investigates the visual habit elaborated to a certain intensity of illumination. Having destroyed the occipital third of the cerebral hemispheres in a rat, the author finds that even the speed of forming the visual habit is not diminished compared with normal animals. If, however, the same habit already existed in a normal animal before removal of the occipital part of the hemispheres, then the habit disappears and must be formed anew. From this the author draws the risky and somewhat inconceivable conclusion that in general the process of learning does not depend on the site of lesion, whereas the mnemonic trace or engram has a definite localization. But the matter is much more simple. As is known, the occipital lobes are the location of a special visual region, to which the stimuli from the eyes come first of all and where they enter into functional connections with one another for the formation of complex visual stimulations, as well as into direct conditioned connections with the various activities of the organism. But since the visual fibres extend much farther than the occipital lobes, probably over the entire mass of the hemispheres, beyond these special lobes they serve for the establishment of conditioned connections with the various activities of the organism, only in the form of more or less elementary visual stimuli. And should Lashley form a habit not to the intensity of light but to a certain object,

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* K. S. Lashley, "The Relation Between Cerebral Mass, Learning, and Retention," *Jour. Comp. Neur.*, Vol. 41, No. 1, 1926. "The Retention of Motor Habits after Destruction of the So-Called Motor Areas in Primates," *Archives of Neurology and Psychiatry*, Vol. 12, 1924. (*Note by I. P. Pavlov.*)

this habit would disappear with the extirpation of the occipital lobes and would not reappear; thus, there would be no difference between the site of formation of the habit and the place of the mnemonic trace.

In the other research Lashley carried out experiments on the cortical motor region of monkeys. The motor habit does not disappear with the removal of this region. From this he draws the conclusion that the region has no relation to the given habit. But, first of all, in his three experiments he does not extirpate the motor region fully, and probably the remaining parts are still sufficient for a mechanical habit of the given complexity. He excludes this probability not by experiment, but by reasoning. Then, it is possible that in addition to the highly specialized motor region, ascertained by electrical stimulation, there is a less specialized and more extended region. These two considerations necessitate a considerable complication of the mechanical tasks. Finally, why did the author not blind his animals? For there is no doubt that vision also played a part in the formation of the habit, and stimulation of the motor apparatus located below might occur through the visual cortical fibres as well. We have a striking example of this in ataxic patients[118] in cases of tabes dorsalis. The ataxic patient can stand on one leg with his eyes open, but falls when his eyes are closed. Consequently, in the first case he replaces the kinesthetic fibres by the visual ones.

And here again, under the influence of the favourite negative attitude towards detailed localization, there is discontinuation of further necessary experimentation.

Let us now turn to the other experiments and arguments of the author aimed directly against the theory of reflexes. Analysing various adequate stimuli, the author states that most likely one and the same receptor cells do not take part in forming and reproducing a habit, and that this is most obvious in pattern vision. But, in the first place, we see objects, i.e., we receive certain combined visual stimulations with the help of each part of the retina, and not of the whole

retina at once. The same applies to the projection of the retina in the cortex. Consequently, this is the reason why there is no definite connection between the given receptor cells and a definite reaction. Only when we study an object in detail, do we make temporary use of the fovea centralis,[119] and usually each part of the retina serves for a corresponding reaction to the given object. This principle is likewise true for the projection of the retina in the cortex. In the second place, as far as the identity of the reaction is concerned, in the case of a geometrical white figure on a black background or with the relations of brightness reversed, and in cases when the geometrical figures are replaced by corresponding contour outlines, and even partial outlines—this identity can be explained by what has just been said; on the other hand, this phenomenon was thoroughly studied long ago, and it means that at first the most general features of the stimuli act, and then, gradually, under the influence of special conditions, a further analysis takes place and more special components of the stimuli begin to act. In the given case, it is the combinations of white and black points alone, without exact relations and dispositions, that first act as stimuli. And this is demonstrated by the fact that in the course of further special experiments a white figure on a black background could, undoubtedly, be differentiated from a black figure on a white background, i.e., the mutual relationship of black and white would become a special stimulus. The same applies to the replacement of a geometrical figure by a contour outline, etc. All these are but stages of analysis, i.e., more and more detailed elements of the stimuli gradually become stimuli themselves.

Concerning the group of reactions, i.e., the motor apparatus, the author points out that the rat proceeds in the right direction in the maze, in spite of the fact that sometimes it runs very quickly, sometimes moves slowly, and finally, when its cerebellum is injured, makes circular movements. And this, in the author's judgement, is an argument against the existence of any definite connection between the

stimulus and a given reaction. However, the rat constantly moves forward and makes turns to the right and to the left with the help of the same muscles in all the cases mentioned here, the rest are additional movements determined by other additional stimuli. Further, in the case of the exclusion of muscles by paralysis during the elaboration of a habit, and their subsequent use after curing the paralysis, it is necessary to find out why and where the paralysis arose. For we have a vast series of co-ordinated centres, extending from the end of the spinal cord up to the cerebral hemispheres, and conductive fibres may reach out to all of them from the hemispheres. Further, we know that every time we think of a movement, we actually produce it in an abortive way. Consequently, a process of innervation is possible, although actually it does not manifest itself. Then, if the stimulation cannot be realized through the shortest way, it must, in accordance with the principles of summation and irradiation, pass to the neighbouring points. Have we not known for a long time that a decapitated frog, when removing acid placed on the thigh of one of its extremities by using the foot of the same extremity, if unable to do so because of amputation of that foot, will use the other foot after a few unsuccessful attempts with the disabled extremity?

The author's reference to the absence of stereotype in certain forms of movement, for example, in the building of nests by birds, is also based on misunderstanding. The phenomenon of individual adaptation exists throughout the animal world. And this is precisely the conditioned reflex, the conditioned reaction which is effected on the principle of simultaneity. Finally, the author's allusion to the uniformity of grammatical forms fully accords with the above-mentioned fact of systematization taking place in the nervous processes of the cortical activity. This is precisely the combination or fusion of structure with function. And if we do not now have a clear idea of how this occurs, this is only because we do not yet possess complete knowledge of the structure and of the mechanism of the dynamic processes.

I consider it superfluous to dwell further on the author's arguments against the significance of structure in the central nervous system. The common feature of all his arguments is that he disregards the already known complexity of this structure, and even more so, its probable complexity; he constantly simplifies it in a preconceived way, reducing it to the simplest scheme of a physiological text-book, which merely shows the indispensable connection between the stimulation and its effect—and nothing more.

What then does the author propose in place of the theory of reflexes which he rejects? Nothing except extremely remote and fully unjustified analogies. Is it possible to seek a solution of the problem of the higher brain mechanism by references to the tissue of sponges and hydroids or to embryonal tissue, when in the higher part of the brain of higher animals, including man, we have the summit of differentiation of living matter? In any case, admitting absolute freedom of hypothesis, we have the right to demand of the author at least a preliminary and elementary programme of definite problems for successful experimentation on this subject in the immediate future, a programme offering more than the theory of reflexes and capable of vigorously advancing the problem of cerebral functions. But the author has no such programme. A truly scientific theory must embrace not only all the existing material, but open up a wide possibility for further research and, so to speak, unlimited experimentation.

This is precisely the position in which the theory of reflexes now finds itself. Who will deny the extreme, almost inconceivable complexity of the structure of the central nervous system in its highest representative, the human brain, and the necessity for a more profound study of it by improved methods? On the other hand, the human mind still remains overwhelmed by the mystery of its own activity.

The theory of reflexes seeks to provide a possible solution for both one and the other, to elucidate the remarkable mechanism, so difficult to conceive, of this most extraordinary

instrument. The possibility of experimentation on the brain, and especially on its higher part, with the help of the reflex theory with its requirements of constant determination and unceasing analysis and synthesis of the underlying phenomena, is truly unlimited. This I have felt and seen continuously in the course of the past thirty years, and the further, the more distinctly.

Since this is my first appearance in psychological literature, it seems to me opportune, on the one hand, to dwell on certain tendencies in psychology, which, in my opinion, do not agree with the aims of successful investigation, and on the other hand, to emphasize with greater force my point of view on the subject of our common research.

I am an empirical psychologist and I know psychological literature only from the few manuals and the insignificant number—in comparison with the available material—of psychological articles which I have read. But throughout my entire conscious life I have been and still am a constant observer and analyser of myself and others within the range of life which is accessible to me, including belles-lettres and genre painting. I decidedly reject and dislike any theory claiming a complete inclusion of all that constitutes our subjective world, but that does not mean that I refuse to analyse it, or attempt to interpret it simply, in its individual points. This interpretation must consist in bringing its separate phenomena into accord with the data of our modern positive knowledge in the field of natural sciences. For this purpose it is necessary constantly to endeavour to apply in a most thorough way these data to every particular phenomenon. It is my conviction that a purely physiological interpretation of much of what was previously called psychical activity has gained sure ground, and that in the analysis of the behaviour of higher animals, man included, every effort should be legitimately made to interpret phenomena in a purely physiological way, on the basis of established physiological processes. However, it is clear to me that many psychologists, so to speak, jealously protect the behaviour

of animals and man from purely physiological explanations, constantly ignore them and do not try to apply any of them in an objective way.

To prove what I have just said I shall take only two very simple cases: one mine and the other Prof. Koehler's,[120] although I could cite many much more complex cases.

When we were trying out the method of feeding an animal from a distance during experimentation, we employed different procedures, and among them the following: in front of the dog we always placed an empty plate to which a metal tube led from a vessel containing the powder of dried meat and bread with which we usually fed our animals at the time of experimentation. Located at the junction of the vessel and the tube was a valve which could be opened at the right moment by means of pneumatic transmission; in this way a portion of the powder came down the tube and spilt out into the plate where it was eaten by the animal. The valve did not work very well and when the tube was shaken some of the powder would fall into the plate. The dog quickly learned to make use of this, that is, to shake out the powder without assistance. And such shaking occurred almost constantly whenever the dog, while eating its portion of food, brushed against the tube. This, of course, is exactly what takes place when the dog is being trained to give the paw. In our experimental case the laboratory surroundings in general taught the dog, while here only a part of the surroundings—man. In the latter case the words "paw," "give," etc., the tactile stimulation which arises when the dog lifts its paw, the kinesthetic stimulation which accompanies this movement, and, finally, the visual stimulation coming from the trainer, are accompanied by feeding, i.e., they are linked up with an alimenary conditioned stimulus. What happened in the above-mentioned case is exactly the same thing: the noise of the shaking tube, the tactile stimulation from the contact with the tube, the kinesthetic stimulation resulting from knocking against the tube and finally the sight of the tube—all were likewise connected with the

act of eating, with excitation of the alimentary centre. This, naturally, occurred through the principle of association by simultaneity and was a conditioned reflex. Two additional physiological facts are clearly manifested here. In the first place, the definite kinesthetic stimulation is connected, in the given case, probably in a conditioned way (while in the lower parts of the central nervous system in an unconditioned way), with performing the movement that originated the kinesthetic stimulation. In the second place, when two nervous centres are connected or joined together, the nervous processes move from one to the other in both directions. If we accept as absolutely valid the principle of one-way conduction of the nervous processes in all points of the central nervous system, then in the given case we shall have to assume an additional reverse connection between these points, i.e., to admit the existence of an additional neurone connecting them. When the lifting of the paw is followed by presentation of food, the excitation undoubtedly proceeds from the kinesthetic point to the alimentary centre. But when the connection is established and the dog, being in a state of alimentary excitation, gives the paw itself, the excitation obviously runs in the opposite direction.

I cannot interpret this fact in any other way. Why this is only a simple association, as psychologists usually affirm, and not an act of comprehension, of sagacity, even though elementary, remains obscure to me.

The other example is taken by me from W. Koehler's book (*Intelligenzprüfungen an Menschenaffen*) and also relates to a dog. The dog is kept in a large cage placed in an open space. Two opposite walls of the cage are solid and nothing can be seen through them. Of the other two, one is a grille through which free open space is visible, and the other has an open door. The dog stands in the cage facing the grille, and a piece of meat is placed in front of it at some distance. The moment the dog sees the meat it turns around, walks through the open door, around the cage and takes the meat. But if the meat is placed close to the grille, the dog vainly

jostles about, trying to get to the meat through the grille, and does not make use of the door. What does this mean? Koehler makes no attempt to solve this question. But with the help of conditioned reflexes we can easily comprehend the matter. When the meat is near the grille, it strongly stimulates the dog's olfactory centre, and the latter, in accordance with the law of negative induction, greatly inhibits all the other analysers, all other parts of the cerebral hemispheres, and thus the traces of the door and of the roundabout way remain inhibited, or, in subjective terminology, the dog has temporarily forgotten them. In the first case, in the absence of a strong olfactory stimulation, these traces are either only slightly inhibited or not inhibited at all, and lead the dog more correctly to its aim. At any rate this interpretation fully deserves further exact verification in an experimental way. Should it be confirmed, it would be possible experimentally to reproduce the mechanism of our reverie, of strong concentration of thought on something, when we do not see or hear what is going on around us, or, what is the same, to reproduce the mechanism of the so-called blindness under the influence of passion.

I am sure that persistent experimentation will elucidate a number of other and more complex cases of the behaviour of animals and man from the point of view of many established rules of the higher nervous activity.

The second point, on which I shall dwell, concerns the significance of the aim and purpose in psychological research. It seems to me that with regard to this point different things are always confused.

Before us is the grandiose fact of the evolution of nature from its initial state in the form of nebulae in infinite space all the way to human beings on our planet, an evolution which, roughly speaking, passed through the following phases: the solar systems, the planetary systems, the inanimate and animate part of nature on earth. The phases of evolution in the form of phylogeny and ontogeny are most strikingly seen in living matter. We still do not know, and

probably shall not know for a long time, either the general law of evolution, or all its successive phases. But observing its manifestations, we replace in an anthropomorphic, subjective way, both in general and in particular, the knowledge of the law by the terms "purpose," "intention," i.e., we merely repeat the fact without adding anything to our actual knowledge of it. But a genuine study of the separate systems of which nature consists, including man, implies a mere statement both of the internal and external conditions of existence of these systems, in other words, the study of their mechanism; to thrust into this investigation the idea of purpose in general, means to confuse different things, to hinder the fruitful investigation which is now accessible to us. In the study of each particular system the idea of possible purpose is not the final aim; it can only serve as an additional help, as a method of scientific hypothesis favouring the formulation of new problems, varying experiments, just as we do when we acquaint ourselves with a machine which is new to us and which is the work of human hands.

The question of freedom of will, naturally, is bound up with this point. Undoubtedly this question is of great vital importance. But it seems to me that it is possible to discuss it in a strictly scientific way (within the limits of modern exact natural science), and at the same time without contradicting the feelings common to all men and without involving confusion in its vital formulation.

Man, of course, is a system (roughly speaking, a machine), and like every other system in nature is governed by the inevitable laws common to all nature; but it is a system which, within the present range of our scientific vision, is unique for its supreme power of self-regulation. Among the products of human hands we already know a number of machines with various systems of auto-regulation. From this point of view, the method of studying the human system is exactly the same as that of any other system; it includes decomposition into constituent parts,

study of the importance of each part, study of the connections between the parts, study of relations with the external environment, and finally, on this basis, interpretation of its general functioning, as well as regulation, if this is within human possibility. But our system is a highly auto-regulating system, capable of maintaining, rehabilitating, repairing and even improving itself. The chief, the strongest and the lasting impression gained from the study of the higher nervous activity by our method is the extraordinary plasticity of this activity, its immense potentialities; nothing is immobile, unyielding; everything can always be attained, changed for the better, if only the proper conditions are created.

A system (machine) on the one hand, and man, with all his ideals, aspirations and achievements, on the other—what a terribly discordant comparison this seems at first glance. But is this really so? And does not it follow from our own point of view that man is the supreme creation of nature, the highest embodiment of the resources of infinite nature, the realization of her mighty and still unexplored laws? Is not this enough to enhance the dignity of man, to afford him the deepest satisfaction? And practically everything vital is retained that is implied in the idea of free will, with its personal, social and civic responsibility; for me there remains the possibility, and hence also the obligation, to know myself, and using this knowledge, always to maintain myself at the highest possible level of my abilities. Do not social and civic duties and requirements constitute conditions which are demanded of my system and which must evoke in it corresponding reactions favouring its integrity and perfection?

DYNAMIC STEREOTYPY OF THE HIGHER PART OF THE BRAIN[121]

Countless stimuli, different in nature and intensity, reach the cerebral hemispheres both from the external world and the internal medium of the organism itself. Whereas some of them are merely investigated (the orienting reflex), others evoke highly diverse conditioned and unconditioned effects. They all meet, come together, interact, and they must, finally, become systematized, equilibrated, and form, so to speak, a dynamic stereotype.

What truly grandiose work!

However, this work is accessible to detailed and exact investigation, naturally, at first, under simplified conditions. We are studying this activity on a system of conditioned reflexes, mainly alimentary, in the course of experimentation on dogs. This system consists of a series of positive stimuli, acting on different receptors and having different intensities, as well as of negative stimuli.

Since all these stimulations leave, after their action, more or less profound traces, precise and constant effects of the stimuli can be obtained in the system with the greatest ease and most rapidly only when the intervals between the stimuli are invariable and when the stimuli are applied in a strictly definite order, i.e., when they are externally stereotyped. In the end, a dynamic stereotype develops, i.e., a co-ordinated, equilibrated system of internal processes. The evoking and establishing of a dynamic stereotype is a nervous task of extremely variable intensity which, on the one hand, depends on the complexity of the system of stimuli,

and, on the other hand, on the individuality and state of the animal.

I take as an example one of the extreme cases. (Virzhikovsky's experiments.) In a stereotyped system well elaborated in an animal of the strong nervous type and composed of positive stimuli of various intensities, as well as of negative conditioned stimuli, we introduce a new stimulus, but in the following peculiar way: we apply it four times in the course of the experiment after different stimuli, i.e., at different moments of the experiment, but accompany it by an unconditioned stimulus only at the fourth application. The reflex is soon manifested and begins to develop; but this process is accompanied by extreme excitation of the animal, which tries to break away from the stand, throws off the apparatus attached to it, and howls. The previous positive stimuli lose their effect; the animal even rejects food, and it becomes more and more difficult to bring it to the experimental chamber and to emplace it in the stand. This painful state persists for two or three months, until at last the task is solved by the animal; the stereotype establishes itself: the first three applications of the new stimulus no longer produce any positive action, being inhibited, only the last (fourth) application is effective, and the animal fully calms down.

Thus, the establishment of the new dynamic stereotype requires an enormous expenditure of nervous energy, which can be sustained only by a strong nervous type.

But let us continue our experiment. When the first task is accomplished, we offer the animal another. Now also the first three applications of the new stimulus are accompanied by feeding, which means that the animal must transform them from inhibitory into positive ones. A state of excitation is again observed in the animal, but this time less intense and lasting for a shorter period, until all applications of the new stimulus begin to produce one and the same positive effect. Thus, the reshaping of the stereotype again requires certain effort. But since food has been pre-

sented to the dog this time, it is now not a question of inhibition of the alimentary excitation, as was probably the case, even though partially, with the first task, but precisely of the establishment of a new dynamic stereotype in the cerebral hemispheres. This is realized more rapidly and easily because the second task, obviously, is much simpler. Of course, simpler systems of conditioned reflexes are elaborated by the same animal with greater ease, in any case without marked efforts on its part.

To me it would be strange not to regard this nervous work as mental activity, just because the psychologists ascribe to the dog only associative activity.

But such is the state of affairs only with dogs possessing strong and equilibrated nervous systems. The picture is altogether different with dogs possessing a strong, but unequilibrated nervous system, dogs which are more or less feeble, sick, worn out, or aged. There are dogs which from the very outset, despite favourable conditions, are incapable of elaborating a dynamic stereotype—the effects of the conditioned stimuli constantly vary from experiment to experiment in a chaotic way. In this case we come to the assistance of the animal by simplifying the system of reflexes, for example, by reducing their number to two positive ones. A mere change in the sequence of the old stimuli in the course of the experiment also represents a difficult task, which in our experimental conditions leads sometimes to complete temporary discontinuance of the reflex conditioned activity. Even the maintenance of an already elaborated system calls for definite effort, which some dogs can endure only if there are intervals of two or three days in the course of experimentation, i.e., if the dogs are allowed regular rest. Without intervals the effect of the conditioned reflexes shows most irregular fluctuations.

The establishment of a stereotype in the cortical processes clearly manifests itself also in the absence of the real stimuli by means of which it was formed (experiments of Krzhyshkovsky, Kupalov, Asratyan, Skipin and others).

Here is one of these interesting experiments. We elaborate in the animal a series of positive conditioned reflexes of different intensities, as well as of negative reflexes, applied at regular intervals and always in a strict sequence. Later, when in one of the experiments we apply only one of the positive (preferably weak) stimuli, the following occurs. The effect throughout the experiment shows the same fluctuations as exhibited by the whole system of different stimuli. The old stereotype persists for a time and then gives way to a new one, i.e., the repetition of one stimulus in the end produces a uniform effect. But the role of the old stereotype, if well established, does not end here. If the last stimulus is not applied for a time and then is tried once more, we get not a new stereotype but again the old one. Consequently, there takes place a certain supersession of stereotypes and a sort of rivalry between them.

Another, still more interesting, phenomenon is also observed. We have a stereotype elaborated from different stimuli. If alongside it a hypnotic state sets in in the dog during the experiment (this state easily develops in some dogs when a single stimulus, and, moreover, a weak one, is applied), then the stimulus now applied singly instead of the previous system, reproduces by its effects the entire system, but in a distorted way: a weak effect is obtained from the previously strong stimuli, and a strong effect—from the weak stimuli, i.e., there arises the paradoxical phase. As is known, we established this phase long ago for stimuli of different intensities in a hypnotic state. Thus, in the given case the dynamic stereotype is combined with the hypnotic state.

I believe that there are sufficient grounds for assuming that the above-described physiological processes in the cerebral hemispheres conform to what we use to designate subjectively as our *senses*, both in the general form of positive and negative senses, and in the form of their numerous nuances and variations due to different combinations and

intensities. Among these are the senses of difficulty and facility, gaiety and fatigue, satisfaction and chagrin, joy, triumph, despair, etc. It seems to me that the painful senses, which often accompany a change in the habitual mode of life, an interruption of customary work, loss of close relations or friends, to say nothing of a mental crisis and collapse of beliefs, are, to a considerable degree, physiologically caused precisely by the change, the disturbance of the old dynamic stereotype and the difficulty of elaborating a new one.

In particularly intense and durable cases a morbid melancholy may also arise. In this connection I vividly recall the following fact which happened in my student days. We, three schoolmates, entered the university, and influenced by our literary inspirer at the time[122] chose the faculty of natural sciences. Thus, we began to study chemistry, botany, etc., i.e., we first began to learn definite facts. While two of us got used to this study, the third, whose favourite subject at school had been history and who had been very fond of writing compositions concerning the causes and consequences of various historical events, became more and more depressed and ended in deep melancholy and persistent attempts to commit suicide. His recovery was attained in the following way. We, his friends, at first almost forcibly, made him attend the lectures in the law faculty. After attending a few of them a marked improvement set in, until finally the normal state was fully recovered. Afterwards he entered the law faculty, graduated and was absolutely normal all his life. From our conversations with him before and during the disorder we learned that at school he had acquired the habit of freely relating one phenomenon to another, there being no serious obstacles in this respect, and that he had tried to do the same now, in studying the natural sciences. However, the implacable facts constantly opposed this trend and did not permit doing what could be easily done with the purely verbal material.

Repeated failure brought on his bad mood and finally led to a morbid form of melancholy.

Similarly, when we offered our dogs difficult tasks, i.e., when we demanded of them the formation of new and complicated stereotypes, we met not only with the painful state described at the beginning of this address, we were also able to produce chronic nervous diseases—neuroses, which subsequently had to be treated.

CONCERNING THE POSSIBILITY OF FUSION OF THE SUBJECTIVE AND THE OBJECTIVE[123]

The physiology of the higher nervous activity developed before our eyes when the physiologist first began to study systematically by the objective method of conditioned reflexes the normal activity of the cerebral cortex and of the adjacent subcortex—the special apparatus of relations between the entire organism and the surrounding medium—and to establish the fundamental laws of this activity, i.e., to study it exactly in the same way as he studies the apparatus of digestion, circulation of the blood, and other subjects.

Since then there have gradually developed ever-increasing possibilities to superimpose the phenomena of the subjective world on the physiological nervous relations, in other words, to merge them. This was inconceivable when the experiments of the physiologist were confined to artificial stimulation of different cortical points and extirpation of different parts of the cortex in animals. On the contrary, at that time there was a strange situation. Two branches of human knowledge which deal with the activity of one and the same organ of the animal and human organism (who can dispute this now?) remained more or less isolated from each other and sometimes even regarded their independence of each other as a matter of principle. As a result, the physiology of the higher part of the brain stagnated for a long time; as for psychology, it could not even work out a common language to designate the phenomena investigated by it, although numerous attempts to elaborate a ter-

minology that would be adopted by all psychologists were made.[124] Now the situation has radically changed, especially for the physiologists. Vast prospects for observation and experimentation open up before them. The psychologists will at last gain common firm ground—a natural system of the basic phenomena studied by them, a system that will enable them to classify more easily the interminable chaos of human emotions. There is coming into being, and there is bound to be realized, a natural union, a fusion of the psychological with the physiological, of the subjective with the objective. The problem that has troubled human thought so long will find its *real* solution. And the urgent task of science in the near future is to contribute to this in every possible way.

Naturally, the possibilities for this fusion are provided most of all by cases of disorder of the human brain when distortion of the human subjective world is linked, obviously, with anatomical and physiological disturbances of the higher part of the brain.

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X

EXPERIMENTAL PATHOLOGY OF THE HIGHER NERVOUS ACTIVITY

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First I should like to say a few introductory words concerning the complicated fate of our work in the sphere of physiology and pathology of the higher nervous activity, assuming that the adjectives "higher nervous" conform to the adjective "psychical."

Thirty-five years ago I was engaged in the investigation of digestion—previously a special subject of mine—and among other things I investigated the so-called "psychical secretion" of saliva. Intending to subject it to further analysis, I soon became convinced that if we adopted the psychological standpoint, that is, if we started guessing what the dog feels, thinks, etc., nothing would come of it and no exact knowledge could be obtained. It was then that I first decided to treat this psychical phenomenon, this "psychical salivation" as objectively, that is, solely from without, as everything else is studied in physiology. Soon Dr. Tolochinov became my associate and we began this work together. Helped by numerous collaborators we have been carrying on this work incessantly for the last thirty-five years.

At the outset the work was marked by a slight but interesting occurrence in our laboratory life. When I decided to continue the work along those lines, one of my collaborators, a very clever and alert young man who had worked with me on another, ordinary physiological subject, expressed his astonishment and even indignation. "How is

that?" he said, "For goodness' sake! Is it conceivable to study psychical activity on dogs, and in the laboratory?" And this, as it appeared subsequently, was very significant. Twelve years later, when I travelled to London for the jubilee celebrations of the Royal Society, I met the leading British neurophysiologist Sherrington. "You know," he said to me, "your conditioned reflexes would hardly be popular in England, since they have a materialistic flavour."[126]

Well, and how do matters stand at present? I must tell you that these first impressions of our new work are still typical of the attitude to it of a considerable part of the educated public; and because of this work I am regarded by many as a very odious person.

Now, what about science? Here, too, the situation is far from being definite. True, in England, the country with which Sherrington tried to frighten me, there is an altogether different situation. There, the theory of the conditioned reflexes is now taught in all schools. It has been widely recognized also in the United States. But this is a long way from being the case in all countries. In Germany, for instance, the approach towards this theory is far from being such. Not very long ago a German professor of physiology visited Kharkov; in the course of a conversation with Professor Volborth—one of my former assistants—about conditioned reflexes, he plainly stated that this was "keine Physiologie."

It should be added that in general physiologists still cannot exactly determine the proper place of conditioned reflexes in the text-books on physiology. It seems to me that these reflexes must be rightfully advanced to the fore when expounding the physiology of the cerebral hemispheres, since they represent the normal, objectively established work of these hemispheres. Analytical data accumulated up to the present time by means of stimulations, extirpation[127] and other methods of investigating the cerebral cortex, must follow, naturally, the description of the normal activity.

I do not know what impression our modern physiology of conditioned reflexes expounded by Prof. Podkopayev has made on you, but in submitting the pathology of these reflexes, I make so bold as to think that you will see for yourselves how expedient and fruitful our method of treating the subject is. That is why I deemed it necessary to begin with this short introduction.

Now for the subject itself. I am very glad that Prof. Podkopayev delivered his lectures on the physiology of conditioned reflexes, prior to me, before this very audience; this relieves me of the need to make any preliminary explanations. I take it for granted that all of you are in possession of the basic physiological data, and so I shall proceed directly to an exposition of the purely pathological facts.

The nervous activity, as all physicians are aware, consists of two mechanisms, or two processes—excitatory and inhibitory. With regard to these two processes we distinguish three fundamental elements, namely, the strength of both the excitatory and inhibitory nervous processes, the mobility of these processes—their inertness or lability—and finally, the equilibrium between these processes.

Certainly, the entire normal higher nervous activity, or in the usual terminology, the psychical activity, not only of animals, but also of man, is based on the normal course of these processes with their inherent properties. At least our experiments with dogs, our usual objects of investigation, have convinced us that all their intricate and highly complex relations with the surrounding world fully come within the bounds of our research into the above-mentioned processes and their properties; they are comprehended by us to the extent permitted by the possibilities of our experiments.

We can divert all these processes with their basic properties from their normal path and cause them to become pathological. For this purpose we have quite definite methods at our disposal. There are three of these methods—overstrain of the excitatory process, overstrain of the inhib-

itory process and overstrain of the mobility of the nervous processes. As to the latter method, it should be pointed out that the expression "overstrain of the mobility of the nervous processes" is actually used by me for the first time; usually we refer to it as collision of the excitatory and inhibitory processes.

How to weaken the excitatory process, to make it pathological? For this purpose it is necessary to act upon the cell, within which the excitatory process is produced, by an external agent of a very great, extraordinary strength; by doing so we overstrain the work of the cell, its excitatory process, as a result of which the latter becomes pathological.

In a similar way, that is, by overstrain, the inhibitory process can also be made pathological.

You already know how we obtain inhibition by means of negative conditioned stimuli. Let us suppose that a given conditioned inhibitory stimulus has constantly evoked in its cell inhibition of half a minute duration and that the cell has endured it very well. I then expressly prolong the action of the same stimulus for five or ten minutes. A strong cell is able to sustain it, but in a weak cell the inhibition breaks down; the work of the cell becomes pathological and changes in different ways.

Finally, the third method. It is possible to make both the excitatory and inhibitory processes pathological by means of abruptly transforming, without any intervals, the inhibitory state of the cell into a state of excitation, or vice versa. We usually refer to this as a collision between the excitatory and inhibitory processes. It is obvious that a certain amount of time is required for a corresponding change in the activity of the cerebral cells, just as it is required for any other activity. Under such collisions only those cells may remain unaffected and intact where the basic nervous processes are strong and especially where these processes are highly labile.

Now, what results from the action of these morbific methods? How does deviation from the normal occur? How

is the pathological state of the cells originated? A general weakening of the cell takes place. As to the excitatory process, the cell becomes incapable of performing the work which it performed previously, i.e., the limit of its working capacity decreases, and this manifests itself in the following pathological phenomena.

You are aware that if we have before us an absolutely normal cell and that if we apply external agents of different physical strength as conditioned stimuli, the conditioned effects of these stimuli more or less correspond to their physical strength.

Now, if we break this cell down, i.e., if we overstrain it and thus make it pathological, its relation to the stimuli becomes different. In some cases conditioned positive stimuli of different physical strength produce an equal effect, and then we say that this is the equalization phase of the cell's activity. In other cases, when the weakening of the cell, i.e., the decrease in the limit of its working capacity, is progressing, a state sets in in which strong stimuli produce a lesser effect than weak ones; this is the paradoxical phase. Finally, a further disturbance of the cell's activity manifests itself in the fact that the cell no longer responds to the positive stimulus, whereas the inhibitory stimulus produces a positive effect; we have termed this phase the ultra-paradoxical phase.

Besides the decline in the limit of working capacity, i.e., the weakening of the excitatory process in the cell, there can also be observed other changes of the excitatory process. One of the most striking of these—particularly interesting and particularly applicable in neurology and psychiatry—is the inert state of the excitatory process, i.e., a state in which the excitatory process becomes more tenacious, persistent, and gives way more slowly to normally arising inhibitory influences.

I shall dwell for a while on inertness. The excitatory process normally, even in healthy people, varies not only in strength, but also in another respect—in mobility. With

some people the excitatory process is less mobile, i.e., it is more susceptible to stimulation, reacts more quickly under the influence of stimulation; at the same time, when the stimulation is over, the effect disappears sooner than with other types of normal people.

On this basis we, like Hippocrates, divide the equilibrated, strong animals into two categories—the phlegmatic and the sanguine. The phlegmatics, it follows, will be characterized by a relatively slow development of the excitatory process, the sanguines—by a quick one.

But this is within the bounds of normalcy. If, however, I act on the cell by means of morbific methods, I can make the inertness of its excitatory process excessive and pathological so that its state of excitation becomes exceedingly persistent.

Concerning the pathological changes of the excitatory process the following addition should be made. Two morbid changes in its mobility are observed. One of these I have just mentioned—pathological stagnation. Given other morbific conditions we get a diametrically opposite state of the nerve cell, namely, pathological lability. In neurology this is known as excitatory weakness, i.e., a state in which the cell becomes very alert, very rapidly reacts to stimulation, and at the same time quickly becomes bankrupt and weakens. We call this the state of explosiveness.

In the same way it is possible to break down (in our usual laboratory terminology) the inhibitory process as well, to make it pathological. By means of a sudden, and not gradual, considerable extension of the duration of the inhibitory state in a cell through the action of a corresponding external stimulus, we can greatly weaken the inhibitory function of the cell and almost fully destroy it. It should be pointed out that in this respect the inhibitory process has been investigated to a lesser degree than the excitatory one.

The inhibitory process, too, usually manifests itself in different ways with regard to its mobility. Sometimes it

develops rapidly and just as rapidly vanishes; sometimes, on the contrary, it assumes a more protracted character.

Thus, the inhibitory process is either normally inert or normally labile. However, it can also be brought to a pathological state with regard to inertness. In our laboratory there is a dog which has been exhibiting pathological inertness for the past three years. In this animal under the influence of frequent collisions the positive stimulus began to evoke, instead of the normal excitatory process, an inhibitory one, and of such a persistent nature that although we constantly reinforced it under favourable conditions in the course of the three years, we just could not restore its initial positive effect. Only recently did we find a means of changing this state of affairs, but I shall speak about that at the end.

Thus, you have before you, in general outline, the changes which occur under the action of morbific agents—the change in the excitatory process, the change in the inhibitory process and, hence, as a result, a derangement of the proper correlations between the excitatory and inhibitory processes. But the normal activity of the nervous system is, of course, determined by an equilibrium between these basic processes with their normal properties.

I must tell you that often it is quite easy to obtain a pathological state of the higher nervous activity with the help of the methods I have just mentioned. But, depending on the types of nervous system, one can observe a great difference in the facility with which this pathological state is attained.

In equilibrated and strong animals, i.e., those in which both the excitatory and inhibitory processes are of equal strength and whose lability is normal, it is, of course, likewise possible to produce a nervous disorder; however, it would take considerable time and labour, since it necessitates trying different methods. In excitable and weak animals this is very easily attained. As you already know, we classify as "excitable" that type of animal in which the excitatory process is very strong; the inhibitory process is

probably also considerable, but the two processes do not conform. The excitatory process strongly predominates, and therefore in this type the negative stimuli hardly ever reach zero. This type can be broken down rather easily, i.e., made pathological. As soon as it is offered a series of tasks calling for a considerable degree of inhibition, it becomes quite weak—the animal can no longer discern anything, inhibit anything, i.e., it becomes neurotic.

As regards animals of the weak type, they can be easily made abnormal by all our methods.

The neurotic state manifests itself in the fact that the animal does not properly respond to the conditions in which it exists. This relates both to its laboratory characteristics and general behaviour. With regard to the latter everyone will admit that whereas previously the dog was normal, it is now ill.

In the laboratory we usually apply a system of conditioned reflexes—positive and negative—which are elaborated on the basis of various unconditioned stimuli: positive reflexes to stimuli of different physical strength and different kinds of negative reflexes. This entire system is normally governed by strict rules: the positive effect depends on the strength of stimulation; the inhibitory stimulus produces a greatly diminished or a zero effect, etc. Under the influence of our morbific methods all or many of the normal reactions become weakened and distorted.

The disturbed nervous equilibrium is clearly observed not only by us in the system of conditioned reflexes; our attendants also notice it. The dog obeyed them previously, behaved orderly, it knew where to go when led to an experiment. Now everything has abruptly changed. And the attendants simply say that the dog has become stupid and even has gone mad.

The pictures of neuroses in diseased animals vary considerably owing either to the different intensity of the disorder, or to the appearance in the foreground of this or that pathological symptom. Recently we have observed a partic-

ularly large number of such neuroses and neurotic symptoms on an organically pathological basis, namely, on castrated animals. It goes without saying that castration itself disturbs the normal relations within the nervous system. I shall, therefore, briefly touch on the post-operative state of our dogs, as far as their nervous system is concerned.

One of the most striking of the morbid, neuropathological symptoms appearing almost immediately after castration is an enormous decline of the inhibitory process, of the inhibitory function, so that the dog, which prior to the operation acted in an exemplary manner, in full accordance with the conditions influencing its nervous system, now becomes quite chaotic. Normally one sees day after day an absolutely uniform and perfectly exact system of conditioned reflexes, but after castration no day is similar to another; there is a series of entirely different days and there is no order whatsoever.

One more very important detail manifested itself shortly after castration and surprised even us. In the case of strong types, the action of the animals after castration, as I have just said, is extremely distorted and instead of being strictly regular, becomes chaotic. In the case of weak types the reverse is the case. For some time after the operation the dogs behave better and more orderly than before. True, this different condition exists only temporarily—for one, one and a half or two months. Then the nervous activity in these dogs, too, becomes weakened just as that in strong dogs. I shall revert to this question later and show on what this difference is based and how we interpret it.

Then, after months of entirely chaotic activity a circularity sets in which did not exist before, i.e., the dogs do not work and manifest their system of conditioned reflexes in a disorderly manner constantly, i.e., from day to day, but their activity now periodically changes. It is chaotic for a while and then for a certain period it greatly improves in a spontaneous way and becomes more orderly. And as time goes on, the more distinct this periodicity becomes; the

periods of better work are more frequent and of larger duration, until after some years everything becomes normal. This, obviously, denotes the existence of certain adaptability in the organism.

Of course, since we know the system of endocrine glands, which to a certain degree assist and replace one another, it is conceivable that in time the defect sustained by the organism immediately after castration becomes more or less levelled out. But the return to the apparent normal after castration takes place in different dogs after different periods; with some it occurs after one month, with others it takes years, and there are dogs in which this state has so far not set in at all. This is obviously connected with the initial strength of the nervous system.

It is clear that in these castrated dogs, after their full or partial recovery, it is possible to produce various neuroses much more easily than in absolutely normal dogs, since in the former the equilibrium has already been disturbed, and naturally they are, so to speak, more fragile than normal dogs. Thus, we can produce in them numerous neurotic disturbances by means of the above-mentioned morbific methods.

To a considerable degree the pathological nervous states produced by us conform to the so-called psychogenic diseases in human beings.[128] The same overstrain and the same collisions of the excitatory and inhibitory processes are also encountered in our own lives. For instance, somebody has deeply insulted me and I for some reason or other have not been able to respond to it by corresponding words, or, moreover, by a certain action, with the result that I had to overcome the struggle or conflict between the excitatory and inhibitory processes within myself. And this was repeated more than once. Or let us take another case from the literature of neuroses. A daughter is at the sick-bed of her father whom she loves deeply and who is living his last days; however, she must pretend that everything is all right and that everybody expects his recovery, whereas in reality she is

weighed down by unbearable anguish and sorrow. This often leads to breakdowns, to neuroses.

Indeed, can we find any essential physiological difference between such breakdowns and those which we obtain in our experimental animals by colliding the excitatory and inhibitory processes?

But in addition to these neuroses, there must be, owing to the extreme complexity of our brain in comparison with that of the higher animals, special human neuroses, to which I ascribe psychasthenia[129] and hysteria.[130] These states cannot be produced in dogs, since in cases of this kind the division of the human brain into a higher, purely human part, connected with speech, and a lower part, which, just as in animals, receives the external impressions and directly analyses and synthesizes them in a certain way, makes itself felt. But neurasthenic states of different kinds can be fully reproduced in animals.

In view of the fact that our data seemed to me sufficient for a physiological interpretation of the mechanisms of nervous diseases, I decided, two or three years ago, to visit the neurological and the psychiatric clinics (of course, devoting only a little time to the matter). As far as the neurological clinic is concerned, I can say that practically all the neurotic symptoms and pictures observed there can be understood and connected with our pathophysiological laboratory facts. And this is not only my personal opinion, the opinion of a physiologist, it is also the opinion of neuropathologists who acquainted me with the clinic and who admit that our physiological interpretation of neuroses is not fantastic, that we are really laying a solid foundation for constant contact between our laboratory facts and human neuropathological phenomena.

Before passing to another category of our facts I shall explain a phenomenon which I have mentioned but have not analysed in detail.

Why is it that animals with strong nervous systems immediately after castration become chaotic, and only later,

after a certain time, does their behaviour more or less level out, while animals with weak nervous systems, on the contrary, immediately after castration behave better, in a more regular manner than before castration, and only later become disabled?

We think that this phenomenon should be explained in the following way. Since an animal possesses sex glands, it experiences sexual excitation; consequently, additional impulses come to the brain and tonify it; but the brain is weak. Hence the deficiency in the general nervous activity. With the removal of the sex glands the additional stimuli disappear, the nervous system is eased, and its activity in all other respects assumes a more expedient character. There is nothing fantastic in this explanation. We clearly observe the same in another, more tangible case. The degree of appetite in the experimental dog is of great importance in our system of conditioned reflexes. If you have a strong dog and increase its food excitability by means of a certain method (while performing experiments with alimentary reflexes), then all its conditioned effects are increased. On the contrary, with a weak dog a heightened food excitability usually leads to a decline of the conditioned reflexes, i.e., the additional excitation cannot be endured by the dog, and is accompanied by inhibition, which we, therefore, call protective.

Now I shall proceed to another category of facts. The development of definite pathological states in the nervous system with the aid of our definite methods is, of course, based on the fact that our concept of the mechanism of this system is to a certain degree correct. The power of our knowledge over the nervous system will, of course, appear to much greater advantage if we learn not only to injure the nervous system but also to restore it at will. It will then have been really proved that we have mastered the processes and that we can control them. Actually, this is the case. In many instances we not only bring on disease, but eliminate it with great exactitude, one might say, to

order. Of course, in this case it was necessary, above all, instead of reasoning and searching for various remedies at random, to be guided by the indications of medicine. Thus, bromide plays a very important role with us. But in order to apply this remedy accurately thorough knowledge of the mechanism of its action was necessary.

With regard to bromide we have definitely established, without the least doubt, that its action is quite different from that hitherto assumed and possibly still assumed by pharmacologists. The physiological effect of bromide consists not in decreasing excitability or in weakening the excitatory process, but in intensifying the inhibitory process. Bromide bears a special relation to the inhibitory process, and this can be proved by numerous experiments. Here, for example, is a very simple experiment which we always apply when need arises.

You have an excitable type of dog—the type in which the excitatory process is extremely strong and the inhibitory process relatively weak. Consequently, the dog cannot bring its inhibitory reflexes to a complete zero—its inhibition is insufficient. You administer bromide to the dog and immediately obtain complete inhibition. You often observe in this case also a greater positive effect than previously, before the administration of bromide. But there is another, no less important side to the effect of bromide.

Although bromide has been rightly used as a remedy for nervous diseases for years (I do not know exactly for how many years but not less than sixty or seventy), it is an absolute truth that to this day medicine has not always used this powerful instrument of nervous therapy in a proper way, often committing a very serious error.

You administer bromide in a case of a neurotic state. Let us suppose that the bromide produces no effect. Then you increase the dose thinking that the previous dose was too small. But this is true only in one series of cases. In other cases, and probably in the overwhelming majority of them, the dose must be decreased and not augmented. Often you

must even decrease the dose to a very considerable degree. The gradation of the useful doses of bromide is highly extensive; in our dogs its limits are approximated to a thousandfold. This is absolutely true and we all guarantee it. Consequently, a very important correction must be made in medicine in this respect. If you administer an excessively large dose, you may obtain an injurious instead of a beneficial effect; you may cause the patient serious injury.

There can be no question, of course, that this is true only of dogs, and that with nervous people matters are different. The neuropathologists in our clinic have observed that when they took these facts into consideration it turned out that in many cases successful treatment necessitated not an increase in the doses of bromide but reduction to decigrammes and centigrammes. The general laboratory rule is: the weaker the type of nervous system and the given nervous state, the smaller must be the dose of bromide.

As is also well known in medicine, rest, too, provides a certain curative effect in laboratory neuroses. If a dog has been made neurotic by us, it is often helpful not to work with this dog every day, since a daily system of our conditioned reflexes is undoubtedly a difficult task, which in this state is beyond the dog's strength. As soon as you introduce a regular two or three day recess between the experiments, the nervous system begins to recover.

In some cases it has been observed that rest, as it were, substitutes bromide. Suppose you have a dog whose work after castration is chaotic. You can help it in two ways: either you make it work (that is, you experiment with it) not every day, but once in two or three days, with the result that its work considerably improves; or you administer a suitable dose of bromide which produces the same effect.

It should be pointed out that we are now applying another extremely important method of treatment, but as yet we are not entitled to say definitely that it is an agent of

radical treatment. Still, it is impossible not to pay attention to it and not to look upon it with great hope.

With the help of our morbific methods, which make the whole cerebral cortex pathological, it is also possible to cause a completely isolated region of the cortex to become ill; this is an extremely important and highly impressive fact. Suppose you have a dog with a series of different acoustic conditioned stimuli: beats of the metronome, a noise, a tone, a crackling or a gurgling sound, etc. From all these stimuli it is not difficult to obtain only one which would prove noxious and evoke a sharp deviation from the normal. So long as you apply the other acoustic stimuli, the animal's behaviour is orderly and its work is quite regular. But the moment you touch the point of application of the morbific stimulus, not only is the reaction to it distorted in one degree or another, but thereafter the entire system of conditioned reflexes becomes deranged, and its harmful effect spreads over the whole cerebral cortex. This fact in itself leaves no room for doubts, since it has been frequently produced and is being produced now by many experimenters.

But here I would like to draw your attention to the following. When I enumerated all our sounds, it was obvious that they were of a more or less complex nature. How, then, are we to picture the disorder of the cerebral cortex in relation to separate sounds? It can hardly be assumed that to each sound applied by us there corresponds a particular group of nerve cells receiving the elementary acoustic stimuli of which the sound is formed. It is more probable that in the case of each of our acoustic stimuli it is a question of a dynamic structural complex, whose elements, the corresponding cells, enter also into other dynamic complexes when other complex sounds are applied. And it is the results of the difficulties created by our morbific methods in the processes connecting and systematizing the dynamic complexes that are responsible for the destruction and disturbances in those complexes.

Isolated pathological points can be obtained in all parts of the cerebral hemispheres. Here is an example. You elaborate conditioned positive stimuli from a mechanical stimulation of different spots of the skin. You can obtain such a state when in two points of the skin the excitatory process does not call forth any pathological effect while the third is functionally pathological.

We now have a dog of the excitable type, i.e., one in which the excitatory process is extremely strong but in which the corresponding inhibition is insufficient. This dog has been castrated. Being of a strong type it recovered rather quickly. Since it was excitable much time and effort was required prior to castration to elaborate in it a differentiation to the metronome. For a period after castration our laboratory sustained some trouble: there was a shortage of food for the animals and they became emaciated. Due to the general nervous exhaustion the reflex of our dog to the metronome, which had been complicated by a difficult differentiation, became morbid, while all other conditioned reflexes remained unaffected. As soon as metronomes were applied, normal work with conditioned reflexes became impossible. We tried to exclude the inhibitory metronome as the more difficult one, and to make use only of the positive metronome, but that did not change the picture. Bromide proved ineffective, which, for some unknown reason, is generally the case in disorders of isolated points of the cerebral hemispheres.

Then the question arose whether the same thing would occur in another part, in another analyser of the cerebral hemispheres where the excitatory and the inhibitory processes would collide. In order to obtain an answer to this question we selected the cutaneous region, where we could apply an easier differentiation, i.e., make one spot of the skin positive and another inhibitory. The stimulation of one spot was reinforced by feeding, while that of the other spot was not. The effect was the same. So long as the positive conditioned stimulus alone was being elaborated, the

dog behaved quite normally, and the entire system of reflexes was in order. But as soon as the inhibitory stimulus began to manifest itself, all the reflexes diminished and became distorted; the dog became extremely violent, so that the experimenter could not attach the apparatus to the skin or take it off without the risk of being bitten.

Now I wish to direct your attention to the following interesting phenomenon. When we had such isolated points in the cerebral cortex of other dogs, their harmfulness and morbidness were expressed only in the fact that their stimulation resulted in the derangement or destruction of our entire system; but our observations showed that this was never accompanied by a manifestation of pain in the animals. However, in this case there was a distinct impression that the touch to the skin became painful. How is this phenomenon to be explained?

As a matter of fact the only difficulty during the collision of the excitatory and inhibitory processes was in the brain, and this difficulty made itself felt in the system of conditioned reflexes. What, then, caused the pain in the skin? Apparently this may, and should, be explained in the following way. In a certain point of the cerebral cortex of the dog there arises a considerable difficulty, which must cause pain, just as you feel a kind of heaviness, a very disagreeable sensation in your head when you tackle an extraordinarily difficult problem. We must assume a similar state in our dog. But in the course of these experiments the dog apparently formed a conditioned connection between the attaching of the apparatus to the skin and the difficult state of the cutaneous analyser in the brain; conditionally the dog transfers the struggle against this difficult state in the brain to the moment of skin stimulation, exhibiting resistance to any contact with the skin. However, this is not a hyperaesthesia of the skin. Consequently, this is an extremely interesting case of objectification of an internal cerebral process, a manifestation of the strength of its connection with the stimulation of the skin. As for the brain,

we must assume merely a special kind of heavy sensation in it, a peculiar kind of pain. It is not without reason that psychiatrists have described melancholia as a mental pain, or a cortical pain, the sensation of which differs from the pain caused by wounds or disorders of different parts of the organism.

Thus, for a long time we could not do anything with this dog. At last, however, a favourable way out was found thanks to the good fortune of one of my oldest and most valuable associates, Dr. Petrova. Formerly Petrova worked as a therapist, but later she was enticed into the study of conditioned reflexes and has devoted herself entirely to it for many years. I had an interesting experience in this connection. I must tell you that although I began my professorship as a pharmacologist,[131] I have always had a strong prejudice against introducing several substances at a time into the organism. It always struck me as strange whenever I saw a prescription containing three and more drugs. What a brew! And I had always been against such combinations of pharmaceutical remedies in the physiological analysis of phenomena; in this I proceeded from the principle that the simpler the conditions of the phenomena are, the better the chances for elucidating them. I admitted bromide to our laboratory as a single drug basing myself on medical practice; caffeine was also introduced as a separate stimulant related to the excitatory process But I was always against using them in combination. However, the therapist, being used to combinations, insisted on a trial, and proved to be right. The effect was extraordinary and miraculous. When a mixture of bromide and caffeine was given to the dog mentioned above, the persistent neurosis immediately disappeared without leaving the slightest trace. We acted carefully. Having administered the mixture of bromide and caffeine for two days, we at first tried only the positive mechanical stimulation of the skin. The effect proved to be normal; the animal was absolutely quiet and no derangement of the system of condi-

tioned reflexes was observed. A little later, being encouraged by the results of the trial of the positive stimulus, we applied the negative one. In this case too the effect proved to be the same—there was not the slightest trace of the former morbid reaction.

Post factum it was not difficult for me to build a respective theory. Now I presented the matter to myself in the following way. Certainly it must be assumed that in the overwhelming majority of cases a disorder of the nervous system is a disturbance of the proper correlations between the excitatory and inhibitory processes, as it appeared in the course of application of our morbific methods. Now since we have, so to speak, two levers in the form of pharmaceutical remedies, two communicators towards the two chief apparatus, i.e., towards the two processes of nervous activity, then by putting into action and correspondingly changing the strength now of one, now of the other lever, we have a chance of restoring the disturbed processes to their former place, into their proper correlations.

We have another similar case. I have already mentioned the case of the dog in which the pathological inertness of the inhibitory process lasted for three years, i.e., its positive process became pathological and the positive stimulus turned into an inhibitory one. Although we have been constantly reinforcing this stimulus for three years now, i.e., we have been creating the conditions under which it ought to be positive, we have always had it inhibitory. No matter what we tried—bromide, rest, etc.—nothing helped. Under the influence of the mixture of bromide and caffeine this stimulus which for such a long time produced a morbid reaction, has now assumed a normal positive effect.

In the same dog, parallel with the pathological inertness of the inhibitory process, there was pathological lability of the excitatory process on another stimulus, i.e., it developed its action not gradually but impetuously, in an explosive manner; but a negative phase set in quickly in the course of the excitation. At the first moment of the application of

this conditioned stimulus the dog makes a violent effort to reach the food receptacle and exhibits a profuse salivary secretion, but soon, already in the course of excitation, the salivation stops; when you begin to reinforce the stimulus and offer food, it does not take it and turns away. This pathological phenomenon, too, disappears under the action of our mixture, the morbific stimulus becoming quite normal in its action.

Interesting too is the following fact. We administered the mixture to this dog for ten days and then decided to find out whether the cure was radical. But this was not the case. When we ceased to administer the mixture the old relations returned. Of course, much more time is probably required to eliminate the disturbances entirely. But one can also assume that we really establish correct relations between both processes changing them temporarily, but do not treat the processes themselves, or at least both of them simultaneously. It is clear that should it be the first case, it is a great triumph for therapy. In any event, in the present-day palliative, and possibly future radical treatment by means of a mixture of bromide and caffeine, it is necessary to take into account the extreme precision of the dosage of both drugs, reducing them, especially in the case of caffeine, even to milligrammes.

In conclusion, I shall briefly touch on the question of the application of our laboratory results to the neuropathological and psychiatric clinics. As for the first, there is no doubt that our human neuroses can be explained quite satisfactorily in the light of the laboratory analysis. But it seems to me that in psychiatry, too, certain things have been clarified by our laboratory research.

At present I am writing a series of booklets entitled "Latest Papers on the Physiology and Pathology of the Higher Nervous Activity." Two brief articles published in the last issue have been translated into foreign languages. One of them has already been published in French, the other has been sent to an English psychiatric journal, and it goes

without saying that I eagerly await the reaction of our own and foreign experts.

Now you are aware that in the laboratory we are able to make pathological, and besides, in a functional way, an isolated point of the cerebral cortex, leaving all other points absolutely intact. I wish to make use of this phenomenon of isolated disorders for interpreting a very interesting and very enigmatic psychiatric form, namely paranoia. As is known, paranoia is characterized by the fact that a mentally normal person, who, like all healthy people, reckons with logic and reality, and sometimes may even be gifted, as soon as it comes to one definite subject, distinctly turns into a lunatic, acknowledging neither logic, nor reality. It seems to me that this form can be understood on the basis of our laboratory findings relating to isolated disorders of separate points in the cerebral cortex.

One can hardly dispute that the stereotypies of skeletal movement[132] can and should be understood as the expression of the pathological inertness of the excitatory process in the cortical cells which are connected with movement, and that perseverations[133] should be similarly looked upon only in the cells of speech movement. But at first sight it is more difficult to explain obsessive ideas and paranoia in the same way. However, it seems to me that the understanding of isolated pathological points of the cerebral cortex not only in a purely crude anatomical sense, but also in a structurally-dynamic one (as mentioned above) has eliminated this difficulty to a sufficient degree.

Here is another case of a neurosis which is very close to a psychosis.

In persecution mania the patient sometimes firmly regards as reality that which he fears and wants to avoid. For example, he wants to have a secret and it seems to him that all his secrets are constantly being disclosed in some way. He wants to be alone, and although he is alone in his room and everything lies open before his eyes, he still imagines that somebody else is with him. He wants to be

respected, and it seems to him that at every moment he is being insulted in some way or other by signs, words, or facial expressions. Pierre Janet has described this as feelings of possession, as if somebody is taking hold of the patient.

In my view, this case is based physiologically on the ultra-paradoxical phase, which I have already mentioned and which, as you know, consists of the following.

Suppose we have two metronomes of different frequency which act as conditioned stimuli, one of them with 200 beats per minute being the positive stimulus and the other with 50 beats—the negative one. Now, if the nerve cell becomes pathological or simply falls into a hypnotic state, the effect is reverse: the positive stimulus turns into an inhibitory one, and the inhibitory becomes positive. This is an absolutely exact and constantly recurring laboratory phenomenon. Therefore, I interpret the state of the above patient in the following way: when he wanted to be respected or to remain alone, this was a strong positive stimulus, which evoked in him an opposite idea involuntarily and irresistibly in accordance with the rule of ultra-paradoxicality.

Thus you see that in the field of pathology our method of work, the method of an objective attitude towards the higher phenomena of the nervous activity, is fully justifiable for animals, and the more we apply it the more it is justified. At present we are making, as it seems to me, warrantable attempts to apply the same method to the human higher nervous activity which is usually called psychical activity.

That is all I wanted to tell you.

TYPES OF HIGHER NERVOUS ACTIVITY, THEIR RELATIONSHIP TO NEUROSES AND PSYCHOSES AND THE PHYSIOLOGICAL MECHANISM OF NEUROTIC AND PSYCHOTIC SYMPTOMS[134]

Of the vast material relating to the study of the higher nervous activity in dogs by the method of conditioned reflexes I shall now dwell only upon three points because of their particularly close connection with morbid disturbances of this activity. They are: the strength of the two basic nervous processes—excitation and inhibition—then the correlation of their intensities, or their equilibrium, and finally their mobility. These properties constitute, on the one hand, the basis of the types of higher nervous activity, types which play an important part in the genesis of nervous and so-called mental diseases, and on the other hand, typical changes taking place under pathological states of this activity.

Two thousand years ago the great genius of ancient Greece—the artistic genius, of course, not scientific—was able to discern in the immense diversity of variations of human behaviour its fundamental features in the form of four temperaments. And only now is the study of the higher nervous activity by the method of conditioned reflexes in a position to base this systematization on a physiological foundation.

According to the strength of the excitatory process (i.e., according to the working capacity of the cerebral cells) our dogs were divided into two groups—strong and weak. The strong group, in its turn, was divided into equilibrated and

unequilibrated, depending on the correlations between the intensities of the excitatory and inhibitory processes. And finally the strong and equilibrated dogs were divided, according to the mobility of the processes, into quiet and lively ones. Thus, there are four basic types: the strong and impetuous type, the strong, equilibrated and quiet type, the strong, equilibrated and lively type, and the weak type. And they correspond to the four Greek temperaments—choleric, phlegmatic, sanguine and melancholic. Although there are different gradations of these types, life clearly shows that it is just these combinations that are more frequently met with and bear a more pronounced character. It seems to me that this coincidence of types in animals and human beings is convincing proof that such a systematization conforms to reality.

However, to obtain a full and clear idea of the variations of human behaviour, normal and pathological, it is necessary to add to these types, which are common in man and animals, certain particular, purely human types.

Before the appearance of the family of homo sapiens the contact of the animals with the surrounding world was effected solely by means of direct impressions produced by its various agents which acted on the different receptor mechanisms of the animals and were conducted to the corresponding cells of the central nervous system. They were the sole signals of external objects. In the future human beings there emerged, developed and perfected, signals of the second order, signals of these initial signals, in the shape of speech—spoken, auditory and visible. Ultimately these new signals began to denote everything taken in by human beings directly from the outer, as well as from the inner world; they were used not only in mutual intercourse, but also in self-communion. This predominance of the new signals was conditioned, of course, by the tremendous significance of speech, although words were and remain but second signals of reality. We know, however, that there are large numbers of people who, operating exclusively with

words and failing to base themselves on reality, are ready to draw from these words every possible conclusion and all knowledge, and on this basis to direct their own life as well as the life of others. However, without entering deeper into this important and very broad subject, it is necessary to state that thanks to the two signalling systems, and by virtue of the long-established different modes of life, human beings in the mass have been divided into artistic, thinking and intermediate types. The last-named combines the work of both systems in the requisite degree. This division makes itself felt both in individual human beings and in nations.

Let us pass now to pathology.

In our experiments on animals we constantly obtained convincing proof that chronic pathological derangement of the higher nervous activity under the influence of morbific agents arises with particular ease in the impetuous and the weak types, where it assumes the form of neurosis. Impetuous dogs become almost completely deprived of inhibition; in weak dogs the conditioned reflex activity either fully disappears, or is of a highly chaotic character. Kretschmer, who recognizes only two general types corresponding to our impetuous and weak types, correctly, as far as I can judge, associates the first with the manic-depressive psychosis, and the second with schizophrenia.

Having some very limited clinical experience (during the last three or four years I have regularly visited the nervous and psychiatric clinics) I take the liberty of advancing the following supposition concerning human neuroses. Neurasthenia is a pathological form inherent in the feeble-general and intermediate human types. A hysterical person is the product of the feeble-general type combined with the artistic type, and the psychasthenic (to use the terminology of Pierre Janet) is the product of the feeble-general type combined with the thinking type. In hysterical persons, general weakness, naturally, has a special effect on the second signalling system, which in the artistic type in any case yields pride of place to the first system, while in normally

developed persons the second signalling system is the highest regulator of human behaviour. Hence the chaotic character of the activity of the first signalling system and of the emotional fund in the form of pathological fantasies and unrestrained emotivity with profound destruction of the general nervous equilibrium (sometimes paralyses, at others contractures,[135] or convulsive fits or lethargy) and in particular, synthesis of personality. In psychasthenics the general weakness, naturally, again affects the basic foundation of the correlations between the organism and environment, namely, the first signalling system and the emotional fund. Hence the absence of a sense of reality, continual feeling of inferiority of life, complete inadequacy in life together with constant fruitless and perverted cogitation in the form of obsessions and phobias.[136] This, in general outline, is how I conceive the genesis of neuroses and psychoses in connection with the general and particular types of human higher nervous activity.

Experimental study of pathological changes in the basic processes of the nervous activity of animals makes possible a physiological understanding of the mechanism of the mass of neurotic and psychotic symptoms, both taken separately or as components of certain pathological forms.

Weakening of the excitatory process leads to the predominance of inhibition, both general and diversely partial, in the form of sleep or of a hypnotic state with its numerous phases, of which most characteristic are the paradoxical and ultra-paradoxical phases. This mechanism, I believe, is responsible for a particularly large number of pathological phenomena, such as narcolepsy, cataplexy, catalepsy,[137] feelings of possession—les sentiments d'emprise (according to Pierre Janet), or inversion (according to Kretschmer), catatonia,[138] etc. The weakening of the excitatory process is caused either by its overstrain, or by its collision with the excitatory process.

Under certain laboratory conditions which are not yet quite clear there takes place a change in the *mobility* of the

excitatory process in the form of *pathological lability*. This phenomenon, long known in the clinic under the name of excitatory weakness, consists in an extremely high reactivity or sensitivity of the process followed by its rapid consecutive exhaustion. Our conditioned positive stimulus produces an instantaneous and extraordinary effect, which, however, falls to zero and becomes inhibited already during the normal period of stimulation. We sometimes call this phenomenon explosiveness.

But in our experimental practice we also meet with quite the opposite pathological change in the *mobility* of the excitatory process—with *pathological inertness*. The excitatory process persists despite a prolonged application of conditions, under which normally the excitatory process is superseded by inhibition. The positive stimulus is not susceptible or slightly susceptible to successive inhibition evoked by preceding inhibitory stimuli. This pathological state is in some cases caused by a moderate, but continuously growing intensity of the excitatory process, and in other cases by collisions with the inhibitory process. It is quite natural to attribute the phenomena of stereotypy, obsessive ideas, paranoia, etc., to this pathological inertness of the excitatory process.

The inhibitory process can also be *weakened* either by its overstrain, or by collisions with the excitatory process. This weakening leads to an abnormal predominance of the excitatory process in the form of a derangement of differentiations, retardation and other normal phenomena in which inhibition intervenes; it also manifests itself in the animal's general behaviour in the form of fussiness, impatience and violence, and finally in the form of pathological phenomena, for example, neurasthenic irritability. In man it takes the form of submanic and manic states,[139] etc.

This year phenomena of pathological lability of the inhibitory process have been observed in our animals by my old colleague, Prof. Petrova, who has enriched experimental pathology and therapy of the higher nervous activity with

quite a considerable number of important facts. A dog which previously took its food, placed at the edge of a staircase, with ease, without any hesitation, ceases to do so, hurriedly avoids the food and moves away from the edge. The matter is quite clear. When a normal animal, approaching the edge of a staircase, stops and does not move farther, this means that it is able confidently to hold itself back, as much as is necessary to prevent it from falling down. In our case this retention is exaggerated; the reaction to depth is excessive and keeps the dog, to the detriment of its interests, much farther from the edge of the staircase than is actually necessary. Subjectively this is an obvious state of dread or fear, a phobia of depth. The phobia could be induced, and could be eliminated, i.e., it was under the experimenter's control. The condition responsible for its emergence is what we may call the torture of the inhibitory process. I will demonstrate this fact in a few days' time at the international physiological congress in Leningrad. I think that in many cases persecution mania can also be accounted for by the pathological lability of inhibition.

We have already examined the pathological inertness of the inhibitory process.

A difficult task still remains to be accomplished—it is necessary to determine with precision and in all cases when and in what particular conditions one or another pathological change arises in the basic nervous processes.

FUSION OF PRINCIPAL BRANCHES OF MEDICINE IN MODERN EXPERIMENTATION AS DEMONSTRATED BY THE EXAMPLE OF DIGESTION[140]

Despite the extremely complex nature of biological phenomena, and notwithstanding the difficulty of establishing their true causal relationship, and, consequently, of controlling them, the irresistible pressure of life forced medicine, even in olden times, to take charge of these phenomena, i. e., long before they began to be studied by natural science. And to a degree at least, medicine accomplished this task. At first sight the job seemed to be a huge one and hopelessly difficult, and yet it was partially accomplished. Among the countless possible solutions a few were seized upon as being truly fortunate. This incredible success was determined by two extremely vital conditions: in the first place, it was due to man's incessant and passionate striving for health and life, a striving which arose together with the first man, and, in the second place, to the participation of multitudes, of almost all mankind, in realizing this desire. But if the successes already achieved by medicine are astounding, who can doubt that they are still very insignificant compared with those which will be eventually attained. This, however, will take place not only because medicine utilizes, one might say, at every instance, and will continue in an ever-increasing measure, to use the general achievements of natural science for diagnostic and therapeutic purposes. Were medicine to remain purely practical, it is doubtful if it

would attain complete triumph, since in most of its activity it would be doomed to apply only one instrument of natural science—observation; the other instrument—experimentation—is utilized by medicine with extreme caution and within relatively narrow bounds. But the method of observation is sufficient only for the study of the simpler phenomena. The more complex the phenomenon (and what can be more complex than life?), the greater the need for experiment. Experiment alone crowns the efforts of medicine, experiment limited only by the natural range of the powers of the human mind. Observation discloses in the animal organism numerous phenomena existing side by side and interconnected now profoundly, now indirectly, or accidentally. Confronted with a multitude of different assumptions the mind must *guess* the real nature of this connection. Experiment, as it were, takes the phenomena in hand, sets in motion now one of them, now another, and thus, by means of artificial, simplified combinations, discovers the actual connection between the phenomena. To put it in another way, observation collects that which nature has to offer, whereas experiment takes from her that which it desires. And the power of biological experimentation is truly colossal. This experimentation has created in the course of some seventy or eighty years practically the entire modern, highly developed physiology of the organs of the complex animal. The ordinary educated man, even if he is not yet familiar with biology, upon acquainting himself with the usual, but somewhat more thoroughly arranged course of demonstrative physiology of animals, designed for medical students, would undoubtedly be extremely surprised at discovering the power which the present-day physiologist wields over the complex animal organism. And his surprise would be all the greater upon discovering that this power is the result not of millenniums or centuries, but only of decades.

Before our eyes this triumphant experimentation is steadily extending its power both to pathology and therapy. And

it is difficult to imagine why this power of experimentation should not be equally effective in new branches of research. It seems to me that the remarkable success of modern medicine lies precisely in the fact that it is now in a position to develop all its principal branches by applying the method of experimentation. Bacteriology, in its turn, has given a tremendous impetus to this new tendency. Although pathology came to the laboratory somewhat earlier than bacteriology, the action of the still unknown, but highly pathogenic world of micro-organisms represented a very serious obstacle to experimental pathological investigation. We had in hand only the inanimate causes of the disease, such as mechanical force, heat, etc., but its living causes, the micro-organisms, escaped us. Only when the pathogenic organisms were discovered, did the domain of pathological physiology fully open up before the experimenter, and now nothing can prevent him from investigating almost the entire pathological world in laboratory conditions.

Although the clinic, as a result of its work of thousands of years, established with remarkable precision the forms of various diseases and gave almost the complete morphology of pathological states, although crude pathological anatomy, as well as the more recent microscopic and clinical investigations, collected and continue to collect rich material relating to the inner details of pathological processes, a full analysis and complete knowledge of the mechanism of such processes, of their entire development, can be obtained only by means of experimentation. In this respect the method of pathological anatomy alone is too crude, and the clinic alone, without experimentation, is powerless to penetrate deeply into the complexity of phenomena. Only laboratory experimentation makes it possible to discern in the general picture of a disease that which constitutes the protective mechanisms of the organism, and which compensates for the loss caused by the damage proper; only such experimentation can exactly reveal the inter-dependence of injuries, i.e., distinguish the initial injury from those subsequently

evoked by it. Only this knowledge ensures expedient and effective aid to the diseased organism and precludes the possibility of any extraneous intervention which sometimes brings harm to the organism instead of relief. This—on the one hand. On the other, only experimentation will succeed in investigating and establishing all the real causes of pathological states, since it begins by isolating the cause which it deliberately induces to act. And it is precisely here that medicine is most vulnerable: it is a well-known fact that aetiology is medicine's weakest point. Indeed, do not the causes of the disease usually creep into the organism where they begin to act long before the patient becomes the subject of medical attention? Knowledge of these causes is, naturally, of extreme importance for medicine. In the first place, only when we know the cause, can we effectively struggle against it, and in the second place, what is still more important, prevent its action, its penetration into the organism. Only knowledge of the causes of diseases will turn the present-day medicine into the medicine of the future, i.e., hygiene in the broad sense of the word. In view of the obvious indisputability and significance of all this, one cannot but regret that pathology, being an exclusively experimental science—pathological physiology—has not yet generally taken its proper place, at times serving as an appendix to pathological anatomy and at others being completely lost in the programme of general pathology. The methods of pathological anatomy and those of experimental pathology differ to such a considerable degree that in practice, under university training conditions, they can hardly be applied by one and the same person and in one and the same premises on the principle of equality. It seems to me, on the other hand, that in the so-called general pathology special emphasis must be laid now precisely on experimental pathology, on the analysis of pathological processes by means of experimentation, and not on conclusions and abstractions drawn from facts of special pathology, which are often a mere enumeration, though in a new way, of these

facts. One can hardly base serious scientific hopes on such a verbal treatment of the material of general pathology, especially at a time when such fascinating and fruitful experimental study of the world of pathological phenomena is taking place in the laboratory.

It is easy to imagine the embarrassing position of the physician when, in using a certain therapeutic method against one or another illness, against one or another symptom, he is often absolutely unaware of the effect that this method produces in the organism, and does not know in what way it helps in the given case. What inexactitude and uncertainty of action, what scope for fortuities! In these circumstances the striving of the clinicians to elucidate the mechanism of action of their therapeutic remedies is perfectly understandable; for decades experimentation has been rendering assistance to therapy and the therapeutic methods have been studied in the laboratory, where their action on healthy animals is subjected to analysis. This experimentation mainly concerns chemical medicines, hence the development of experimental pharmacology.

However, little by little the pharmacologist departed from his original purpose until he came to show little or no concern or interest in the therapeutic action of the substances handled by him. Pharmacology, naturally, developed into a department of physiology, studying the effect of chemical agents on the living body and pursuing purely theoretical aims. Actually, of course, there can be no objection to this. But thanks to this circumstance the connection between modern pharmacology and practical medicine, which might be described as the original mission of pharmacological experiment, a reminder of which is preserved to this day in the name—the study of medicines—is in many cases, at the present moment at least, weak and at times even purely scholastic. For example, in many text-books the exposition of the physiological action of one or another medicine is followed by an enumeration of the indications and contra-indications of their therapeutic use, often completely

unrelated to the previously described physiological action. This accounts for the complaints we sometimes get from physicians against modern pharmacology. Both experimenters and physicians would benefit by supplementing pharmacology with elements of experimental therapy. Having to do with the sick as well as the healthy animal, and administering drugs not only for the purpose of observing their effect in general, but also for the purpose of healing the sick animal, the pharmacologist, by way of analysis, will, for his own benefit, broaden and deepen his study of the reactions of the body to a given chemical compound, as well as his study of the body in general, and, for the sake of the physician, elucidate the actual value and real mechanism of action of the therapeutic agent. This necessity, at least so far as the study of the action of medicines on sick animals is concerned, was recognized and proclaimed long ago, the only obstacle to its realization being the difficulty of obtaining the necessary sick animals in laboratory conditions; at present this difficulty has been surmounted to a considerable degree thanks to the achievements of experimental pathology. Only if pharmacology is fused with experimental therapy, as mentioned above, will many therapeutical mirages be dispelled, as they rightly deserve to be; it will, on the other hand, preclude the sad possibility of many drugs being wrongly discarded for the sole reason that the pharmacological analysis of their effect on healthy animals has not yet touched the proper points of investigation or has failed to contact them altogether, since it was dealing exclusively with healthy animals. The programme of experimental therapy will, naturally, include the experimental analysis of various therapeutic means other than chemical agents, means which at present are wholly ignored in the vast course of medical academic education.

There is every reason to hope that we shall witness a tremendous growth of interest on the part of investigators when all the pathological processes, and not only the bac-

terial ones, are subjected in the laboratory to bold, unimpeded, and fully controlled treatment. We can be even more sure that outside bacteriology, no less success awaits the experimenter if he, in the field of therapy, assumes the role of initiator and not of interpreter, as has been the case so far. Some people hoped to bring pharmacology and medicine together by recommending the organization of, and by actually organizing, clinical sections in the pharmacological laboratories. But it seems to me that scientifically it is more logical and from the standpoint of practice more advantageous to create experimental therapeutic laboratories than special pharmacological clinics. Indeed, no matter what name you give the clinic, the patient cannot be subjected to a greater degree of experimentation in it than in any other. At the same time skill and system in the matter of treatment are obligatory for every clinical teacher, since precisely this distinguishes him from the ordinary practical physician. Thus, either the experimenter will be sacrificed to the clinician without any essential benefit, or, on the contrary, the clinician will be sacrificed to the experimenter, because a permanent and proportionate fusion of these two branches of activity is hardly within the bounds of practical realization.

I have come to the end of my paper. Only by passing through the fire of experiment will medicine as a whole become what it should be, namely, a conscious and, hence, always purposefully acting science. Modern surgery affords striking proof of this. What is responsible for its brilliant achievements? Its absolute consciousness of action. Proceeding from the plasticity of the organism and armed with antiseptics and aseptics against its chief enemy, the micro-organisms, surgery treats its subject from a purely mechanical point of view and strictly bases its methods on knowledge of the anatomical structure and physiological importance of this or that part of the body.

I am excited at the thought whether I have succeeded in convincing you of the extremely important role of ex-

perimentation in achieving the aims of practical medicine. But if you are convinced of this then your duty is to contribute in every way to the development of biological experimentation not only by possible personal participation, but also by actively assisting the experimenters in their work. The success of biological, that is, medical experimentation depends on adequate personnel, premises and means.

I draw your attention, gentlemen, to an essential difference between the clinicians and the experimenters. In the sphere of medicine scientists usually originate from the mass of the medical workers; every intelligent, energetic and talented practising physician can participate in the general science of medicine and may become a permanent and outstanding worker in this field. But the experimenters constitute an insignificant group of amateurs; their training and activity is almost impossible without special laboratory conditions. And this obliges you to render every possible help, both at college and beyond its walls, to the young champions of laboratory work who, by their laboratory specialization, take the risk of restricting, for the time being, their chances of success in life.

As is known, many clinicians, both therapeutists and surgeons, often have recourse to laboratory experimentation, now for the purpose of analysing a pathological process, now for elucidating the action of certain therapeutic agents, and for trying out a new surgical method, etc. This practice is, of course, commendable. At present the clinicians feel, even more so than the physiologists, the necessity of analysing in laboratory conditions certain clinical pathological or therapeutic phenomena. That is why in numerous cases they now appear as initiators of experimental pathological and therapeutic investigations. This is their great merit and will remain so. However, for them this activity is not of primary importance; they devote to it only the leisure hours remaining after their principal

therapeutic duty. Meanwhile, laboratory activity requires from any scientific worker full devotion, the sacrifice of all his strength. And I am of the opinion that our specialities (experimental pathology and experimental therapy) should be accorded the most favourable and absolutely independent status, since, broadly speaking, they are, in method and concept, nothing else but physiology. The course of medical sciences should, therefore, everywhere comprise three experimental chairs of physiology, namely, normal, pathological and therapeutic physiology.

XI

PHYSIOLOGY AND PSYCHIATRY

PSYCHIATRY AS AN AUXILIARY TO THE PHYSIOLOGY OF THE CEREBRAL HEMISPHERES[141]

My earlier researches on the circulation of the blood and on digestion led me to the firm conviction that the physiological mode of thinking may derive great help from the study of clinical cases, i.e., from the countless number of diverse pathological variations and combinations of the functions of the human organism. For this reason, during many years of work on the physiology of the cerebral hemispheres I often thought of making use of the world of psychiatric phenomena as an analytical auxiliary to this physiological study. Indeed, instead of applying our usual method which, as a mode of analysis, consists in destroying certain parts of the brain, and is very crude compared with the complexity and delicacy of the mechanism under investigation, one might expect in some cases to achieve a more distinct, precise and detailed decomposition of the work of the brain as a whole into its separate elements, to obtain a delimitation of its functions resulting from pathological causes, which sometimes reach a very high degree of differentiation.

In the summer of 1918 I had at last the opportunity to study a number of cases of insanity. And as it seems to me my former hopes have not been in vain. In some instances, I saw excellent demonstrations of points more or less explained in physiology; in others, new aspects of the work of

the brain were brought to light, new questions and unusual problems for laboratory investigation arose.

My attitude towards the psychiatric material, however, differed greatly from the usual attitude of specialists. Due to a definite inclination of thought developed during years of laboratory practice, I always reasoned on a purely physiological basis and constantly explained to myself the psychical activity of the patients in definite physiological concepts and terms. This did not present any great difficulty for me, since my attention was concentrated not on the details of the subjective state, but on the principal features and phenomena of one or another state of the patient. How this was achieved will be partly seen from the following account.

In this article I shall describe and analyse the symptoms observed in two patients. One was an educated, well-bred girl, twenty-two or twenty-three years old. We find her lying motionless in bed in the hospital garden, her eyes half closed. At our approach she does not speak of her own accord. The physician accompanying me tells me that this is now her usual state. She refuses to eat without assistance and is untidy. She appears to understand our questions about her family, and remembers everything perfectly; she answers correctly, but with great effort and after considerable delay. The patient exhibits a strongly pronounced cataleptic state. She has been ill for years, at times almost fully recovering, at others relapsing and manifesting a considerable variety of symptoms; her present state is one of these relapses.

The second patient is a man aged sixty. He spent twenty-two years of his life in hospital, lying like a living corpse, without the slightest voluntary movement and absolutely speechless; he was fed artificially and was untidy. During the past few years, as he was approaching sixty, he began more and more often to make voluntary movements. At present he is able to get up and go to the lavatory; he talks volubly and quite reasonably, and sometimes eats without

assistance. Recalling his former state, he said that he had been conscious of his surroundings, but had experienced such extreme and insuperable heaviness in his muscles that he could hardly breathe. And this was the reason why he could neither move, eat, nor speak. The disorder began to develop when he was thirty-five. Tonic reflexes[142] were recorded in the history of the case.

How should one assess the physiological state of the two patients?

In order to answer this question let us consider the strongly pronounced motor symptoms which are observed in both cases: the catalepsy of the first patient and the tonic reflexes of the second. When do these symptoms manifest themselves in animals? A long time ago Schiff[143] observed cataleptic phenomena in a rabbit deprived of the cerebral hemispheres. Decerebration[144] introduced by Sherrington is a simple method of obtaining distinct tonic reflexes in cats. Intoxication by certain narcotics, for example, urethan, also produces cataleptic phenomena. In all these cases there occurs an elimination of the activity of the cerebral hemispheres *without the suppression of the lower parts of the brain*; in the first two cases this is due to a specific property of the brain tissue of the given animals, as well as to the freshness of the operation, i.e., to the absence of subsequent reactive phenomena; in the case of intoxication by urethan it is due to the presence in the latter of an ammoniac grouping which produces a stimulating action on the lower motor centres. Such an isolated exclusion of the cerebral hemispheres, the nervous organ of the so-called voluntary movements, reveals the normal activity of the lower parts of the nervous motor apparatus. This activity is designed first of all for equilibrating the organism and its parts in space, which represents the equilibration reflex, always functioning under normal conditions and at the same time constantly disguised by voluntary movements. Thus, catalepsy is a normal and permanent reflex which manifests itself in a distinct and patent way only

when the action of the cerebral hemispheres is excluded as in the above-mentioned case. As for the tonic reflexes, they are the elements of this complex reflex.

Consequently, the existence of the same mechanism must be assumed in our patients, i.e., exclusion of the activity of the cerebral hemispheres. It is clear, however, that here only the activity of the motor region of the cerebral hemispheres is excluded, since our patients, unable to make any voluntary movements or suffering extreme impairment of this function, are able, at the same time, as can be seen or as they themselves acknowledge, to understand perfectly well what they are told; they also remember everything and are conscious of their state, i.e., the work of all the other parts of their cerebral hemispheres is quite satisfactory.

This strictly isolated inhibition of the motor region of the cerebral cortex is observed also in other cases, in other definite states inherent in man and animals. A subject in a certain degree of hypnosis understands your words quite well, remembers them and is willing to do something in connection with the conversation, but he has no power over his skeletal muscles and remains in the posture imparted to him, even though it is uncomfortable and he does not like it. Apparently, this phenomenon is essentially accounted for by a fully isolated inhibition of the motor region of the cerebral cortex, an inhibition which does not spread either over other parts of the hemispheres or to the lower levels of the brain mass. I have often observed a similar state in dogs when I worked in the laboratory with the so-called conditioned reflexes. Jointly with Dr. Voskresensky I studied these relations with particular precision and in a most systematic way on one of our dogs. For weeks and months this dog was often left alone in the room for a long time, strapped in the stand without being subjected to any experimental influence. As a result, the entire environment of the room became for the dog a hypnogenous agent, to the degree that the moment it entered the room, its behaviour immediately changed. Strictly measuring the influence of

this agent by varying the duration of its action we could clearly see the separate phases in the development of the sleeping state. The following phenomena were observed. The so-called conditioned alimentary reflex for sound (association) was elaborated in the dog, i.e., at a definite sound the dog exhibited an alimentary reaction: it secreted saliva and made appropriate movements, licking its lips, turning in the direction from where the food was usually offered, and eating the food the moment it was offered. With the first signs of the sleeping state the conditioned salivary reflex to the sound disappeared, but the motor reflex to the sight of food remained quite normal, i.e., the dog began to eat without any delay. This first phase was followed by another one, which was quite unexpected and of considerable interest. Now the conditioned salivary reflex to the sound reappeared and became intensified with the addition of the natural conditioned stimuli proceeding from the food itself. But the motor reflex was absent—the dog did not take the food, even turned away from it and resisted its forcible introduction. In the following phase—the phase of profound sleep—all reactions to food, naturally, vanished. When the animal was deliberately awakened (by means of strong stimuli) the phases indicated above manifested themselves in reverse order as the sleeping state gave way. The second phase, naturally, could be interpreted as follows: the motor region of the cortex was already embraced by sleep inhibition while all other parts of the cerebral hemispheres still functioned quite satisfactorily and manifested their activity on an organ fully independent of the motor region—on the salivary gland. It is impossible not to see here a complete analogy with a person who is being awakened by you; he understands and even admits that you are rousing him at his own insistent request, but he cannot overcome the influence of sleep, begs you to leave him alone or he becomes angry and even aggressive when you persist in fulfilling his request and continue to disturb his sleep.

The first phase and its replacement by the second when the sleep becomes more profound, can be explained thus: since in our case the entire environment of the room, i.e., all the stimuli affecting the eyes, ears and nose, acts as a soporific agent, the parts of the cerebral hemispheres corresponding to these stimuli were the first to be subjected to sleep inhibition, the latter, though still superficial, was strong enough to suppress the conditioned action of the stimuli. At the same time the soporific influence was not yet sufficient to inhibit the more powerful part of the cortex—the motor region. But when monotonous cutaneous and motor stimuli (due to the limited movement in the stand) were added to the sleep-producing action of the room, sleep inhibition extended also to the motor region. And now this part of the cortex, being the strongest, attracted sleep inhibition from all other parts in accordance with the law of concentration of the nervous process; it thereby once more liberated them temporarily from this inhibition, until with the ever-increasing action of all soporific agents the sleep inhibition embraced all parts of the cerebral hemispheres with an equal and sufficient intensity.

And so we have sufficient grounds for granting the existence in the above-described patients of a concentrated and isolated inhibition of the motor region of the cerebral cortex as a result of the pathogenic cause.

What objections, from the clinical point of view, can be raised against our interpretation of the symptoms in our above-mentioned patients? I shall cite the arguments or the seeming inconsistencies with clinical cases which were pointed out by the psychiatrists when we informed them of the results of our analysis. Some of them were inclined to see in the cases cited by us a state of stupor evoked by strong emotions. But in the first place, this concerns the cause of the symptoms and not their mechanism. Evidently cases of stupor, i.e., of similar cataleptic states, may occur under the influence of strong, unusual stimulations caused

by sounds of extraordinary intensity, by uncommon pictures, etc.; a very strong stimulation of certain parts of the hemispheres may lead to the inhibition of their motor region and thus create favourable conditions for the manifestation of the equilibrating reflex. In the second place, in the above-mentioned patients there are no indications of the existence of such a mechanism, and nothing reveals the presence of any extraordinary stimuli; one of the patients plainly points out the extreme difficulty, the impossibility of voluntary movements.

Further, it was stated that in progressive paralysis[145] a destruction of the cerebral hemispheres is proved even on a pathological anatomical basis, although catalepsy is absent. However, in this case there is no complete elimination of the motor activity of the cerebral hemispheres either. The patients are able to make many voluntary movements though they are badly co-ordinated; besides, they often exhibit phenomena of extreme motor excitability of the cortex in the form of convulsions. Consequently, here the main condition for the manifestation of a pure equilibrating reflex is absent.

Reference was made to cases of thrombosis[146] and haemorrhage in the cerebral hemispheres, which are accompanied by paralysis and not by catalepsy. But again this is not a condition which provokes catalepsy. In these cases one observes the absence of even spinal reflexes. It is clear that the inhibitory action of the destruction extends even to the spinal cord. And this inhibition must manifest itself all the more in the parts of the brain adjacent to the cerebral hemispheres.

Thus, the clinical cases of cerebral diseases do not reveal any actual inconsistencies with our analysis of the pathological state of the patients; therefore, in definite cases the mechanism of the pathological activity of the cerebral hemispheres suggested by us must be acknowledged as quite real. The fact that after more than twenty years of illness the patient described in our second case shows signs of re-

turning to normal state, also leads us to interpret the general symptoms as an inhibition of the motor region of the cortex. This means that all the time his state was of a functional rather than of an organic, pathologico-anatomic nature.

Analysing further the state of our patients we must point to another essential circumstance. Although, according to present-day physiology, the cortical motor elements controlling different movements (skeletal, verbal, ocular, etc.), are localized in different parts of the cerebral hemispheres or, so to speak, scattered over them, nevertheless, in our patients they are all united by a common inhibition contrary to all other elements of the hemispheres which remain at the same time more or less free. This leads us to the important conclusion that all the motor elements possess common features in respect of their structure or chemical constitution, or, most probably, of both. Therefore, their relation to the cause originating the pathological symptoms is the same, and in this respect they differ from other cortical elements—visual, auditory, etc. This difference between certain elements of the cortex naturally manifests itself also in the above-mentioned phases of hypnosis and sleep when, influenced by one and the same cause, some elements are in one state and others in a different state.*

Let us now answer the following question: what cause actually determines the given symptoms? Different assumptions are, of course, possible. There may be a definite toxic

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* This difference between the cellular elements of the cerebral cortex must be regarded as being incontestable, especially since in the physiology of the peripheral nerves we constantly meet with a strongly pronounced individuality (excitability, relative strength, etc.) of the nerve fibres (and of their peripheral endings) of different functions. This individuality underlies the methods by means of which the differentiation of these different fibres of one and the same anatomic trunk can be effected. Let us recall, for example, the methods used in separating vasoconstrictor from vasodilator fibres. (*Note by I. P. Pavlov.*)

action, the sphere of influence of which is naturally limited by the individual peculiarities of the separate cortical elements that have just been mentioned. One can also assume exhaustion of the elements of the cortex resulting either from the general exhaustion of the organism, or only from overfatigue of the brain, from exhaustion concentrated in definite elements of the brain either because of the predominant part of these elements in the work producing the exhaustion, or again as a result of their specific nature. Finally, there is the possibility of direct or indirect (the last resulting from local changes in blood circulation or in general nutrition) reflex influences which may effect injuriously and also in an elective manner different elements of the cortex. Hence, in different cases, in spite of the similarity or even identity of the mechanism of the given complex of symptoms, the causes producing them may not be the same.

Finally, the following question is also of definite interest to us: what is the explanation for the case of our second patient, in whom the inhibition of the motor region of the cerebral cortex, having remained for twenty years almost at the same level of intensity, at last began drastically to diminish? This can be accounted for only by the patient's age. Approaching the age of sixty, when a sharp decline in the strength of the organism and the process of its aging usually becomes pronounced, he began to return to his normal state. How is this connection to be interpreted? If a certain toxic agent acted in this case, then, with the senile transformation of the body's chemism, there could take place a weakening, diminution of the agent producing this action. If the principal cause of the disease was chronic exhaustion of the nervous substance, then, with the senile transformation of the brain (lesser reactivity and lesser functional destructibility of the brain which is manifested in a sharp weakening of the memory for current events), this cause could now be less pronounced. Since sleep and hypnosis

should be regarded as a kind of special inhibition, it may be admitted that our second patient presented an example of chronic partial sleep or hypnosis. With the advent of old age there is in evidence a relatively greater decline of the inhibitory processes, expressed in senile talkativeness, fantasticality, and in extreme cases, in dotage. In view of this, the recovery of the patient may be attributed to the senile decline of the inhibitory process.

It can hardly be disputed, I think, that the physiological analysis of the above cases raises before the physiology of the brain many new problems which can be investigated in the laboratory.

AN ATTEMPT OF A PHYSIOLOGIST TO DIGRESS INTO THE DOMAIN OF PSYCHIATRY[147]

In the course of the past thirty years I, together with my numerous colleagues, have been predominantly engaged in studying the activity of the higher parts of the brain, mainly the cerebral hemispheres; this study has been carried out on the basis of a strictly objective method, the method of the so-called conditioned reflexes. We have collected very considerable material relating not only to the normal activity of the above-mentioned parts of the brain, but to a certain degree also to their pathology and therapy. We are now in a position to produce obvious experimental neuroses in our experimental animals (dogs) and to treat them; and it is not impossible, in our opinion, to produce in the same animals states somewhat analogous to the human psychoses. It was this that induced me to make closer acquaintance with psychiatry, of which almost no traces have remained in my memory since my student days in the medical faculty. Thanks to the kindness of my medical colleagues, and especially of Prof. P. A. Ostankov and Dr. I. O. Narbutovich, I am now able systematically to observe different forms of mental disorders. Schizophrenia was the first disorder observed and studied by me. Here my attention was attracted, on the one hand, by the symptoms of apathy, torpor, inactivity, stereotype movements, and on the other hand, by playfulness, exaggerated familiarity, childish behaviour in general, which had not been peculiar to these patients before the onset of the disease (hebephrenia[148] and catatonia).

How can this be explained from the physiological point of view? Is it possible physiologically to generalize these phenomena and to find their common mechanism?

For this purpose it is necessary first of all to consider the facts obtained by the method of conditioned reflexes. This study has provided us with abundant data, particularly relating to the inhibitory process and its physiological and pathological significance.

Inhibition, which together with excitation constantly takes part in the diverse activity of the animal in its wakeful state, also guards the extremely reactive cells of the organism, the cells of the cerebral cortex; it protects them from highly strenuous work under the action of very strong stimuli, or even under the prolonged repetition of weak stimuli; it also ensures the necessary rest for the cells in the form of sleep after their daily normal work.

We have established the indubitable fact that sleep is inhibition, which irradiates over the hemispheres and descends along the brain to a certain level. Besides, we have been in a position to study on our animals also the intermediate phases between wakefulness and complete sleep—the hypnotic phases. These phases have been regarded by us, on the one hand, as different degrees of extensity of inhibition, i.e., of a larger or smaller extent of its irradiation over various areas of the hemispheres, as well as over various parts of the brain, and, on the other hand, as different degrees of intensity of inhibition in the form of different depth of inhibition in one and the same point. It is clear that owing to the tremendous complexity of the human brain, the diversity of separate hypnotic phenomena in man is much greater than in animals. It is possible, however, that some hypnotic phenomena are for one reason or another more manifest in animals than in man, especially since even the manifestations of human hypnosis vary considerably, depending on the peculiar features of the individual and the method of hypnotization. And so, taking into consideration the full complex of symptoms of hypnosis, I shall further deal

with hypnotic phenomena observed both in man and in our animals.

Observing the above-mentioned schizophrenic symptoms I have come to the conclusion that they are an expression of a chronic hypnotic state, which I shall try to substantiate in my further exposition. Of course, apathy, dullness, inactivity, etc., are not in themselves proof of the hypnotic state of the patients, but at the same time they will not conflict in any way with this conclusion, provided my thesis is confirmed by a further comparison of more specific symptoms.

I shall first of all cite the following fact. Apathy and torpor are usually ascertained in a patient when he does not react to the questions addressed to him and gives the impression of being absolutely indifferent to them. However, if the same questions are asked not in a loud voice and not with the usual intensity, but in a low voice and in quiet surroundings, the patient reacts immediately with proper answers. This is a highly characteristic hypnotic phenomenon, to which, in my opinion, constant and proper attention is not being paid. And it is to be regretted that up to now the clinic, as far as I know, has no special term to designate this essential and important symptom as has been done with other symptoms. In our animals this symptom is one of the most frequent and persistent signs of the onset of hypnosis. In our experiments we constantly meet with the so-called paradoxical phase, when in the course of the given experiment or in one of its phases strong conditioned stimuli lose their usual action, while weak stimuli evoke in the animal a perfectly normal effect. In the well-known case of a five-year sleep, or properly speaking, hypnosis, described by Pierre Janet, the author made intellectual contact with his patient solely on the basis of this phenomenon. The patient herself emerged from the hypnotic state only at night, when all the daytime stimulations ceased.

Further phenomena of so-called negativism[149] were observed in the analysed patients. Similarly, in our experimental animals negativism is usually in evidence at the

onset of a hypnotic state. In the case of an alimentary reflex when the conditioned stimulus is brought into action, and the food receptacle is placed before the dog, the latter persistently turns away from it. Not without interest is the following detail very clearly observed in a definite phase: when you begin to move the food receptacle away, the dog, on the contrary, reaches for it. And this is repeated several times in succession. But the moment the state of hypnosis is dissipated, the same dog devours the just rejected food. I shall analyse the mechanism of this, as well as other hypnotic symptoms, at another time; for the present I shall use them only as established facts constituting the hypnotic state.

Another symptom of schizophrenia in one of its variations is stereotypy—a persistent and prolonged repetition of definite movements. This, too, is an obvious hypnotic manifestation, and it is clearly observed in some of our dogs. When the dog is in a perfectly cheerful state, after being fed in the case of a conditioned alimentary reflex, it often continues for a certain time to lick the anterior part of its body, usually the breast and the forelegs. With the onset of a hypnotic state the licking assumes an extremely prolonged character and often lasts until the next meal. Certain other movements, effected by the animal at one time or other, are repeated with similar persistence.

Among usual phenomena observed in schizophrenics are the so-called echolalia[150] and echopraxia,[151] i.e., repetition by the patient of the words addressed to him by his interlocutor and the reproduction of gestures made by someone who attracts his attention. As is known, this phenomenon is also usual in hypnotized normal persons, and, it seems to me, manifests itself with particular ease and most frequently in hypnosis evoked by passes. Catalepsy is a very ordinary phenomenon in schizophrenics, consisting in prolonged retention by the patient of different postures, which are easily, i.e., without any resistance of the musculature, imparted to his body by another person; naturally this relates also to

those postures which the patient himself assumes under the influence of certain temporarily acting stimuli. This, too, is a symptom very easily reproduced in normal persons subjected to hypnotism.

A particularly striking, pronounced and tenacious symptom in certain schizophrenics, constituting even a special form of the disease, is catatonia, i.e., a state of rigidity of the skeletal musculature strongly resisting any change in the given disposition of different parts of the body. Catatonia is simply tonic reflexes, as a result of which a hypnotized person can become as inflexible as a solid board.

Finally, it is necessary to include in this group of different variations of central inhibition the symptom of playfulness or silly mannerisms, mostly observed in hebephrenics, as well as the outbursts of aggressive excitation, met with in other schizophrenics in addition to the already mentioned symptoms. All these phenomena closely resemble the initial state of ordinary alcoholic intoxication, and the state peculiar to children and young animals, for example, puppies, when they are waking up, and especially when they are falling asleep. There is every reason to assume that these manifestations result from a developing general inhibition of the cerebral hemispheres; due to this the adjacent subcortex is not only liberated from constant control, from constant inhibition effected by the cerebral hemispheres in an alert state, but, because of the mechanism of positive induction, is even brought to a state of chaotic excitation affecting all its centres. That is why the state of alcoholic intoxication is accompanied now by a causeless and unusual playfulness and joviality, now by excessive sensibility and tearfulness, now by anger, and, in the case of children when they fall asleep, by capriciousness. Particularly typical is a child in the middle of the first year of its life just going off to sleep. You can see on its face a truly caleidoscopic change of diverse expressions reflecting the chaotic activity of the child's primitive subcortex. Similarly the schizophrenic at definite stages and in definite variations of his disease exhibits

this phenomenon now in a protracted form, now in the form of brief outbursts.

In view of what has been said, one can hardly doubt that schizophrenia, in certain of its variations and phases, is actually a chronic hypnosis. The fact that these variations and phases persist for years, cannot serve as a telling argument against this conclusion. Since there has been a case of a five-year sleep (described by Pierre Janet) and even of a twenty-year sleep (observed in Petersburg), why cannot hypnosis be of an equally lasting character, especially since the instances just mentioned must be regarded as states of hypnosis rather than sleep?

What is the reason for the chronic hypnosis of schizophrenics? What is its physiological, and especially pathological, basis? How does it develop and what are its consequences?

In the final analysis, of course, this hypnosis is profoundly based on the weak nervous system, and especially the weakness of the cortical cells. For this weakness various causes, both hereditary and acquired, may be responsible. We shall not touch here on these causes. But naturally, when such a nervous system encounters difficulties, more often in a critical physiological and social period of life, it inevitably becomes exhausted after excessive excitation. But exhaustion is one of the chief physiological impulses for the appearance of inhibition in the capacity of a protective process. Hence chronic hypnosis is inhibition in different degrees of extensity and intensity. Consequently, this state is, on the one hand, pathology, since it prevents the patient from normal activity, and, on the other hand, according to its mechanism, it is still physiology, a physiological remedy, since it protects the cortical cells from the danger of being destroyed as a result of too heavy work. In our laboratory we have now a striking example showing how prolonged inhibition restores normal activity for a time to weak cortical cells. There are reasons to assume that as long as the inhibitory process operates, the cortical cells are not gravely damaged,

their full return to normal is still possible, they can recover from excessive exhaustion and their pathological process remains reversible. Using modern terminology, it is only a functional disease. That this is really the case, is proved by the following fact. According to Krepelin, a leading psychiatrist, of all the forms of schizophrenia the hebephrenic, and especially the catatonic form—which is of a particularly pronounced hypnotic character—show the highest rate of complete recovery (catatonics—up to 15 per cent), which is not observed in other forms, especially the paranoic one.

In conclusion I take the liberty of offering therapeutic advice more of a practical than sentimental character. Although enormous progress has been made since olden times up to our day in the treatment of the mentally ill, still, I think, something remains *to be desired* in this respect. To keep patients, already possessing a certain degree of self-consciousness, together with other, irresponsible patients, who may subject them, on the one hand, to strong stimulations in the form of screams and extraordinary scenes, and, on the other hand, to direct violence, in most cases, means creating conditions which to a still greater extent enfeeble the already weak cortical cells. Moreover, the violation of the patient's human rights, of which he is already conscious and which partly consists in restriction of his freedom, and partly in the fact that the attendants and medical personnel naturally and almost inevitably regard him as an irresponsible person, cannot but strike further heavy blows at the weak cells. Consequently, it is necessary as quickly and as timely as possible to place such mentally diseased in the position of patients suffering from other illnesses which do not offend human dignity so manifestly.

ESSAY ON THE PHYSIOLOGICAL CONCEPT OF THE SYMPTOMATOLOGY OF HYSTERIA[152]

To my dear comrade Alexei Vasilievich Martynov, in honour of his forty years of brilliant scientific, pedagogic and practical work.

The grateful author,

Leningrad, April, 1932

The objective study of the higher nervous activity by the method of conditioned reflexes has made such progress and has been so widened and deepened that it no longer seems very risky to attempt a physiological interpretation and analysis of the complex, pathological picture presented by hysteria in all its manifestations, although hysteria is regarded by clinicians, fully or predominantly, as a mental disease, as a psychogenic reaction to the environment.

Thus, it is at the same time a test which enables one to judge to what degree the theory of conditioned reflexes is entitled to claim a physiological explanation of the so-called psychical phenomena.

Unfortunately, here again it is impossible to do without a physiological introduction, although a brief one. To this day conditioned reflexes are relatively little known even in the country of their origin; besides, the theory of conditioned reflexes is developing so rapidly that many of its important points have not yet been published and will be expounded by me here for the first time.

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The conditioned reflexes continuously accumulated by human beings and animals in the course of their individual life are formed in the cerebral hemispheres, or, in general, in the higher part of the central nervous system. They represent a higher degree of complexity of ordinary unconditioned reflexes, i.e., reflexes which exist in the organization of the central nervous system from the day of birth.

The biological meaning of the conditioned reflexes consists in the fact that the few external stimuli of unconditioned reflexes, given a definite condition (coincidence in time), establish a temporary connection with the countless phenomena of the surrounding medium—signals of those stimuli. Because of this, all the organic activities representing the effects produced by the unconditioned reflexes, establish more delicate and more precise relations with the environment in wider and wider areas. The theory of conditioned reflexes or the physiology of the higher nervous activity studies the laws governing the dynamics of these reflexes both in normal and pathological life.

The activity of the cerebral hemispheres and of the entire central nervous system with its two processes—excitation and inhibition—is subordinated, in our view, to two fundamental laws: the law of irradiation and concentration of each of these processes, and the law of their reciprocal induction. Experiments carried out on the normal activity of the cortex enable us to draw the conclusion that if the intensity of these processes is weak, they at once begin to irradiate from the point of their origin; if their intensity is strong enough, they concentrate, and if it is excessively strong they irradiate again. When the processes concentrate they induce an opposite process both at the periphery during their action, and at the precise point of action upon its termination.

The irradiation of the excitatory process over the entire nervous system gives rise to a summation reflex. In spread-

ing out, the wave of new excitation is summated with the already existing, manifest or latent, local excitation, revealing in the latter case the latent focus of excitation. In the cerebral hemispheres, which are of a more complex structure and possess extreme reactivity and impressionability, the irradiation of the excitatory process leads to the formation of a temporary conditioned connection, a conditioned reflex, association. While the summation reflex represents a momentary, transient phenomenon, the conditioned reflex is a chronic phenomenon, gradually becoming stronger under the above-mentioned condition; it is a characteristic cortical process.

When the excitatory process concentrates in the entire central nervous system we meet with phenomena of inhibition—a manifestation of the law of induction. The point at which the excitation is concentrated is to a greater or lesser extent surrounded by an inhibitory process representing a phenomenon of negative induction. The latter makes itself felt both in the unconditioned and conditioned reflexes. The inhibition develops in full at once; it always arises and persists not only during the excitation by which it has been produced, but even for some time after it. The stronger the excitation and the lower the positive tonus of the surrounding brain mass the more profound, extensive and durable is its action. Negative induction acts both between the small points of the brain and its large parts. We call this inhibition external, passive, and, it can be added, unconditioned. Previously, this well-known phenomenon was termed the struggle of nervous centres, which emphasized the fact that at a particular time a physiological predominance, or, so to speak, priority of one nervous activity over the other takes place.

Along with the inhibition just mentioned the cerebral hemispheres exhibit other kinds or cases of inhibition, although there are grounds to assume that the physicochemical process in all these cases is one and the same. This is, first of all, an inhibition which constantly corrects

the conditioned connection and accordingly restrains the excitatory process, when the signalling conditioned stimulus is not accompanied, in some cases temporarily, by the signalized stimulus, or when it is accompanied by the latter with a considerable delay. This inhibition becoming highly fragmentary, also delimits, differentiates the conditioned positive agents from the countless analogous and related negative agents. It arises of itself in the conditions mentioned above, gradually grows and gains in intensity; it can train and perfect itself. This inhibition can also become connected with any indifferent external stimulus, if the action of the latter coincides for a certain time with the presence of inhibition in the cortex; this stimulus then begins of itself to produce an inhibitory process in the cortex. From what has been said it is clear that this purely cortical inhibition, along with the conditioned connection, plays an important role in the adaptation to the surrounding medium, constantly and expediently analysing the stimulations coming from there. We have named this kind or case of inhibition internal, active inhibition. Generally speaking, the adjective "conditioned" could be accorded it as well. Then another, specific case of inhibition is observed in the cortex. All other conditions being equal, the effect of the conditioned stimulus is, as a rule, proportional to the intensity of the physical strength of the stimulus, but to a definite maximum level (and probably also to a certain minimum level). Beyond this maximum the effect no longer increases; it may sometimes even diminish. We then say that such a stimulus, on reaching this maximum level, begins to produce inhibition, not excitation. We interpret this phenomenon in the following way: the given cortical cell has a definite limit of functional capacity, i.e., of, so to speak, inoffensive, easily reversible functional wear, and the inhibition, which arises in connection with a super-powerful stimulation, does not permit overstepping this limit. The stronger the super-powerful stimuli, the more intense is the inhibition; in this case the effect of stimulation either remains at the maximum level,

which is more often the case, or diminishes somewhat, if the stimulation is too intense. This inhibition could be called transmarginal.

The limit of functional capacity of the cortical cells is not of a constant nature; it may change abruptly, as well as in a chronic way. Inanition, hypnosis, disease and old age lead to a steady decline of this limit; at the same time in the surrounding environment more and more inhibitory stimuli appear which become super-powerful for the given cell. The following important fact must be also pointed out. When the excitability or lability of the cortical cells is augmented in a natural or artificial way, for example, by means of chemical substances, i.e., when a more rapid functional wear of the cortical cells is provoked, an ever-increasing number of stimuli which previously were below maximum or of maximum strength become super-powerful, leading to inhibition and a general decline of the conditioned reflex activity.

The following question remains unsolved: what is the relation between the two latter cases of inhibition and the first universal case of negative induction? If they are simply a modification of the first case, then what is the nature of the modification and how does it occur in relation to the peculiar properties of the cortex? It is probable that transmarginal inhibition is closer and more related to external, passive inhibition than to internal, active inhibition, since it, too, arises at once, and is not elaborated and trained as the latter.

These two kinds of cortical inhibition also move, spread over the brain mass. A very large number of diverse experiments were performed with the special object of studying the movement of the first kind of cortical inhibition—internal inhibition. In these experiments the inhibition spread out as if before the eyes of the experimenters.

There is no doubt that inhibition, while irradiating and deepening, develops different degrees of a hypnotic state, and that spreading from the cerebral hemispheres down-

ward to the utmost over the brain, it produces normal sleep. The diversity and multiplicity of hypnotic stages, which at first can hardly be distinguished from the waking state, strikingly manifest themselves even in our dogs. In respect of the intensity of inhibition the so-called equalization, paradoxical and ultra-paradoxical phases are worth mentioning. Conditioned stimuli of different physical intensity, instead of producing effects in proportion to their intensity, as in the case of the waking state, now produce equal, or even inversely proportional and distorted effects. In rarer cases the distortion of the effects reaches such a degree that only the inhibitory conditioned stimuli produce a positive effect, while the positive stimuli assume an inhibitory action. In respect of the extensity of inhibition there are observed functional dissociations of the cortex, as well as of the rest of the brain, into larger and smaller parts. The motor area of the cortex is particularly often isolated from other areas, and even in this area sometimes a dissociation of functions comes to the fore.

It is a matter of sincere regret that up to the present time the impression produced by these laboratory experiments is weakened due to the rivalry of the so-called sleep centre suggested by clinicians and certain physiologists;[153] meanwhile, the matter can be interpreted in a satisfactory and conciliatory way from the following point of view, which seems to me fully justified by the facts. One can hardly doubt that there are two mechanisms responsible for the onset of sleep, and that it is necessary to distinguish active sleep from passive sleep. Active sleep originates in the cerebral hemispheres and is based on an active process of inhibition, arising in the hemispheres and spreading from there to the lower parts of the brain. Passive sleep results from the diminution or limitation of stimulating impulses reaching the higher parts of the brain (not only the cerebral hemispheres, but also the adjacent subcortex).

The stimulating impulses include, on the one hand, external stimuli, which reach the brain through the medium of

the external receptors, and, on the other hand, internal stimuli, conditioned by the work of the internal organs and transmitted to the higher parts of the brain from the central nervous region regulating the organism's vegetative functions.

The first cases of passive sleep of a particularly pronounced character are Strümpel's well-known clinical case and the analogical, more recent experiment carried out by Prof. A. D. Speransky and V. S. Galkin when, after a peripheral destruction of three receptors—olfactory, auditory and visual—the dog falls into a profound and chronic state of sleep (lasting for weeks and months). The second cases of passive sleep are the clinical cases which lead to the recognition of what clinicians and some experimenters designate as the "centre of sleep."

The physiology of the muscular tissue offers us an example which in this respect is analogous to sleep. Owing to its specific physiological organization, the skeletal muscle only contracts under the influence of its motor nerve, but the relaxation of the muscle is of a passive nature; as to the smooth muscle, its contraction and relaxation are actively effected under the influence of two special nerves—one positive and the other inhibitory.

Just as in the case of concentration of the excitatory process, the concentration of the inhibitory process engenders, by virtue of the law of reciprocal induction, an opposite process, which in the given case is, naturally, a process of excitation. The point of concentration of the inhibition is surrounded, to a greater or lesser extent, by a process of heightened excitability—a manifestation of positive induction. The positive induction makes itself felt in the unconditioned, as well as in the conditioned reflexes. A heightened excitability arises either immediately or after a certain period during which the inhibition gradually concentrates; it persists not only for the duration of the inhibition, but for some time after its disappearance and in certain cases long after it. The positive induction manifests

itself both between small points of the cortex and large parts of the brain.

I shall dwell now on some points of the physiology of the higher nervous activity which are of importance for physiological analysis of the symptomatology of hysteria.

The connection between the organism and the surrounding medium through conditioned signalling agents is the more perfect the more these agents are analysed and synthesized by the cerebral hemispheres in conformity with the extreme complexity and continuous fluctuations of the environment. The synthesis is effected through the process of conditioned connection. The analysis, the differentiation of positive conditioned agents from inhibitory ones is based on the process of reciprocal induction; the separation of different positive agents, i.e., of agents related to different unconditioned reflexes, is accomplished by a process of concentration (new experiments by Rickman). Thus, precise analysis requires a sufficient intensity both of the inhibitory and excitatory processes.

Further, of particular significance for the physiological study of hysteria are our data relating to the types of nervous system. First of all, we distinguish very strong animals, but unequilibrated, in which the inhibitory process always lags to a certain degree and, consequently, does not conform to the excitatory process. When these animals are confronted with difficult nervous tasks calling for considerable inhibition, they almost fully lose their inhibitory function (special neurosis) and become painfully restless; in some cases this restless state is periodically superseded by depression and drowsiness. In their general behaviour animals of this category are aggressive, provocative, and lacking in self-control. We call such dogs excitable or choleric. Next comes the type of strong and at the same time equilibrated animals, in which both processes are of equal strength; because of this, it is difficult and sometimes even impossible to induce neuroses in such animals by means of complex nervous tasks. This type assumes two

forms—the quiet (phlegmatic) and the very lively (sanguine) forms. Finally, there is the weak inhibitable type, in which both processes are insufficient, particularly and more often the inhibitory process. It is this type which specially furnishes experimental neuroses, reproduced in them with extreme ease. Animals of this type are cowardly; they are constantly in a state of uneasiness or display excessive fussiness and impatience. They are incapable of enduring strong external agents acting as positive conditioned stimuli, any considerable normal excitation in general (alimentary, sexual, etc.), even a slight intensity (continuation) of the inhibitory process, and still less a collision of the nervous processes, any complex system of conditioned reflexes and finally any change in the stereotype of the conditioned reflex activity. In all these cases they exhibit a weakened and chaotic conditioned reflex activity and very often fall into different phases of hypnosis. Moreover, in these animals separate, even very small, points of the cerebral hemispheres can be easily rendered pathological, and when adequate stimuli affect such points a rapid and drastic decline of the general conditioned reflex activity takes place. Although the general behaviour of these animals is such that they cannot always be described as melancholic, nevertheless there is every reason to include them in the category of melancholic animals, i.e., those in which the vital manifestations are in many cases constantly suppressed and inhibited. In our exposition of the types of nervous system we implied, when we spoke of the equilibrium between excitation and inhibition, the so-called internal inhibition. In the weak type, with its weak internal inhibition, the external inhibition (negative induction) is, on the contrary, highly predominant and, above all, determines the entire external behaviour of the animal. Hence this type is called weak, inhibitable.

In concluding the physiological part of this article, I must point to the following circumstance which is of particular importance for the comprehension of some of the spe-

cial symptoms of hysteria. There are sufficient grounds to assume that centripetal, afferent impulses produced by each element and moment of movement reach the cerebral cortex (motor region) not only from the skeletal motor apparatus, which makes possible an exact cortical regulation of the skeletal movements, but also from other organs and even separate tissues; because of this, the cortical regulation of the latter is likewise possible. At the present time, conditioning, which must be related to the higher part of the central nervous system, assumes greater biological significance since the possibility of conditioned leucocytoses, immunity and other various organic processes, has been demonstrated, even though we do not yet know the exact nervous connections participating, directly or indirectly, in this phenomenon. But this possibility of cortical influence is deliberately utilized and is revealed by us in very rare cases under exceptional artificial or abnormal conditions. This is explained by the fact that, on the one hand, the autoregulation of the activity of other organs and tissues, apart from the skeletal motor apparatus, is chiefly effected in the lower parts of the central nervous system, and, on the other hand, is disguised by the fundamental activity of the cerebral hemispheres aimed at regulating the most complex relations with the surrounding medium.

2

Let us turn now to hysteria.

Concerning the general concepts of hysteria held by the clinicians, some of them give a fundamental general characteristic of the pathological state and some bring forward certain particularly pronounced traits or symptoms of this state. Some clinicians speak, as it were, of a return to instinctive, i.e., emotional and even reflex life; others attribute the disorder to suggestibility, explaining the entire behaviour of hysterical persons and the so-called stigmata of

hysteria (analgesia, paralyses, etc.) by suggestion and auto-suggestion. Certain clinicians advance to the foreground the desire to be ill, to take refuge in illness; others regard as particularly important the manifestation of fantasticism, the absence of a real perception of life; still others look on the disease as chronic hypnosis, and finally there are clinicians who ascribe it to a reduced capacity for psychical synthesis or to split personality. I believe that all these concepts taken together fully cover the entire syndrome and the entire nature of hysteria.

First of all, we must consider as a generally recognized fact that hysteria results from a weak nervous system. Pierre Janet plainly states that hysteria is one of an immense group of mental illnesses caused by weakness and cerebral inanition.[154] If that is so, then the above characteristic—taking into account that the weakness mainly relates to the higher part of the central nervous system and especially to the cerebral hemispheres as its most reactive part—becomes comprehensible in the light of the physiology of the central nervous system and of its higher part as now presented by the theory of conditioned reflexes.

Usually the cerebral hemispheres which represent the highest organ of correlations between the organism and the surrounding medium and hence the constant controller of the executive functions of the organism, always exert influence on the adjacent parts of the brain with their instinct and reflex activity. From this it follows that the elimination or weakening of the activity of the cerebral hemispheres must necessarily lead to a more or less chaotic activity of the subcortex devoid of the right measure and of adequacy to the given surroundings. This is a well-known physiological fact which manifests itself in animals after the extirpation of the cerebral hemispheres, in adults when they are in different states of narcotization and in children when they fall asleep. Thus, using the above-mentioned physiological terms, the alert, active state of the cerebral hemispheres, manifested in the unceasing analysis and synthesis of exter-

nal stimuli, of the influences of the surrounding medium, negatively induces the subcortex, i.e., inhibits its activity as a whole, liberating in a selective way only the activity needed by conditions of place and time. On the contrary, an inhibited state of the hemispheres liberates or positively induces the subcortex, i.e., strengthens its general activity. Consequently, there are adequate physiological grounds for the occurrence of various affective outbursts and convulsive fits in hysterical persons under acute and abrupt inhibition of the cortex resulting from unendurable stimulations—and such stimulations are not infrequent in the case of a weak cortex. These outbursts and fits are sometimes expressed in more or less definite instinctive and reflex activities and sometimes in absolutely chaotic forms, depending on the varying localization of inhibition over the cortex and the adjacent or more distant subcortex.

But this is an extreme and active expression of the pathological state. When the inhibition spreads further down the brain, we witness another extreme, but passive state of the organism of the hysterical person in the form of profound hypnosis and, in the end, of complete sleep lasting for hours and even for days (lethargy). This difference between the extreme states is probably determined not only by various degrees of weakness of the excitatory and inhibitory processes in the cortex, but also by the force correlations between the cortex and subcortex, which sometimes vary in an acute or chronic way in one and the same individual, and sometimes are also related to different individuals.

This varying chronic weakness of the cortex, apart from being the cause of the extraordinary and extreme states of the organism just described, invariably conditions also the permanent peculiar state of hysterical persons—their emotivity.

Although our life and that of animals is directed by the basic tendencies of the organism—alimentary, sexual, aggressive, investigatory, etc. (functions performed by the subcortex adjacent to the cerebral hemispheres), neverthe-

less, for the purpose of co-ordinating and realizing all these tendencies, indispensably in connection with the general conditions of life, there is a special part of the central nervous system; this part moderates each particular tendency, harmonizes them and ensures their most rational realization in the conditions of the surrounding medium. These are, of course, the cerebral hemispheres. Thus, there are two ways of action. In the first place it is the way of rational action which is effected after, so to speak, a preliminary (though sometimes almost instantaneous) investigation of the given tendency by the cerebral hemispheres and its transformation, in the requisite measure and at the appropriate moment, into a corresponding motor act or behaviour with the help of the cortical motor region. It is, in the second place, the way of affective, passionate action, realized (perhaps even directly through the subcortical connections) under the influence of the given tendency alone, without the above-mentioned preliminary control. In hysterical persons the latter way of action predominates in most cases, and its nervous mechanism is quite clear. The tendency arises under the influence of external or internal stimulation and evokes the activity of a corresponding point or region of the cerebral hemispheres. Under the influence of emotion and due to the irradiation from the subcortex, this point becomes extremely charged. If the cortex is weak, this is sufficient to provoke a strong and greatly extended negative induction, which excludes any control, any influence of all other parts of the cerebral hemispheres. And it is precisely these parts which locate the representation of other tendencies and of the surrounding medium, the traces of previous stimulations and emotions, the acquired experience. This is joined by another mechanism. The strong excitation produced by emotion intensifies the excitability of the cortex; this rapidly leads the excitation of the cortex to the limit of its functional capacity and exceeds it. Consequently, negative induction is joined by transmarginal inhibition. Hence, a hysterical person lives to a greater or lesser degree

not a rational but an emotive life, and is directed not by the cortical, but subcortical activity.

Suggestibility and auto-suggestibility are directly connected with this mechanism of hysteria. What are suggestion and auto-suggestion? They are a concentrated excitation of a definite point or region of the cerebral hemispheres in the form of a definite excitation, sensation or its trace—an idea now called forth by emotions, i.e., excited from the subcortex, now produced abruptly from the outside, now by means of internal connections, associations—an excitation which acquires a predominant, undue and irresistible significance. It exists and acts, i.e., passes over into movement, into one or another motor act, not because it is maintained by various associations, that is, connections with many present and past stimuli, sensations and ideas—this would produce resolute and sensible action, such as is usual with a normal strong cortex—but because in a weak cortex with a low, weak tone this concentrated excitation is accompanied by a strong negative induction which detaches and isolates it from all indispensable extraneous influences. This is the mechanism of hypnotic and post-hypnotic suggestion. During hypnosis we observe even in a normal and strong cortex a lowered positive tone owing to irradiated inhibition.* When the word or command of the hypnotist is directed to a definite point of such a cortex as a stimulus, the latter concentrates the excitatory process in a corresponding point and is immediately followed by negative induction, which, meeting little resistance on its way, spreads over the entire cortex; thanks to this, the word or

* Despite the very rich material accumulated by the physiology of the nervous system in general, and by the theory of conditioned reflexes in particular, the question of the relation between excitation and inhibition still baffles solution. Is it one and the same process interchanging under definite conditions, or a couple, strongly knit together, which, as it were, revolves under certain conditions and shows, to a greater or lesser extent, or even in full, now one of its sides, now the other? (*Note by I. P. Pavlov.*)

command is completely isolated from all influences and becomes an absolute, irresistible stimulus, continuing to operate even subsequently, when the individual returns to an alert state.

Exactly the same as to its mechanism, but in lesser degree, takes place constantly and spontaneously in old age when the excitatory process in the cortex undergoes a natural decline. In a still strong brain the internal or external excitation, concentrating, even though considerably (but not excessively, as in exceptional cases), in a definite point or region of the cortex, is naturally accompanied by negative induction, which, because of the strength of the cortex, does not represent complete and widely spread inhibiti n. Therefore, along with the predominant excitation, some other concomitant excitations act, which evoke corresponding reflexes, especially old and fixed ones, or the so-called automatic reflexes. Usually our behaviour consists not of isolated, but complex reactions, corresponding to the constant complexity of the surroundings. The picture is altogether different in old age. When concentrating on a certain excitation, we exclude by means of negative induction the action of all other extraneous but simultaneous stimulations, and that is why we often act not in compliance with the given conditions, i.e., our reaction to the entire surroundings remains incomplete. Here is a simple illustration. I look at the object which I need, I take it in my hand but at the same time do not notice all or practically all other objects surrounding and adjoining it; as a result I knock against the other objects, derange them without any need, etc. This is described, erroneously, as senile distraction, whereas, on the contrary, it is concentration, but involuntary, passive and defective. Precisely for the same reason an old man, who thinks of something or talks to somebody while putting on his outdoor clothes, sometimes forgets to put on his hat, takes one object instead of another, etc.

As a result of constant extraneous and involuntary suggestions and auto-suggestions, the life of a hysterical per-

son is overcharged with extraordinary and peculiar manifestations.

To begin with, let us take the case of war hysteria which was thoroughly studied during the world war. Being a permanent and serious menace of death, war, of course, is one of the most natural incentives to fear. Fear has definite physiological symptoms which in individuals with a strong nervous system either do not manifest themselves at all, being suppressed, or quickly disappear; in persons with a weak nervous system they are of a more prolonged character, with the result that such persons are no longer able to participate in military operations and thus are discharged from the obligation of exposing their life to danger. These persistent symptoms could also disappear of themselves with the lapse of time, but in a weak nervous system, precisely because of its weakness, a mechanism sets in which maintains them. The persistent symptoms of fear and the resulting temporary safety thus coincide in time and, by virtue of the law of conditioned reflexes, must become associated, interconnected. Hence, the sensation and representation of these symptoms assume a positive emotional shade and, naturally, are repeatedly reproduced. Then, on the one hand, according to the law of irradiation and summation, they, acting from the cortex, maintain and reinforce the lower centres of the reflex symptoms of fear, and, on the other hand, being emotionally charged, they are accompanied, in a weak cortex, by an intense negative induction, and thus exclude the influence of other representations which could oppose the feeling of conditioned agreeableness or desirability of these symptoms. We, therefore, have not sufficient grounds to affirm that this case represents a deliberate simulation of symptoms. In effect, it is a case of fatal physiological relations.

But a hysterical person displays a multitude of similar cases even in his everyday life. Not only the horrors of war, but many other dangers (fire, railway accidents, etc.), numerous life shocks, such as the loss of close relations or

friends, unfaithful love and other deceptions encountered, the loss of property, collapse of convictions and beliefs, etc., and in general difficult conditions of life—unhappy marriage, poverty, violation of self-respect, and so forth—all these factors produce in a weak individual, at once or eventually, violent reactions accompanied by different abnormal somatic symptoms. Many of these symptoms, which appear at the moment of strong excitation, are impressed in the cortex for a long time or forever, just as are many strong stimulations in normal persons (kinesthetic stimulations included). But other symptoms, which in a normal subject can be effaced with the lapse of time—whether because of fear of their abnormality, inconvenience, direct harmfulness and merely indecency, or, the reverse, because they are advantageous or simply interesting—become more and more intense, extended (through irradiation) and stable, owing to the same mechanism as in the case of the war hysteria mentioned earlier, as well as to their emotional reinforcement. Naturally, in a weak subject, an invalid in life, unable to win by positive qualities respect, attention and favour of other people, the latter motive acts most and contributes to the prolongation and fixation of the morbid symptoms. Hence, one of the most striking features of hysteria is the desire to be ill, to take refuge in illness.

Along with positive symptoms, there are negative ones, which are produced in the central nervous system not by the process of excitation, but by the inhibition process, for instance, analgesia and paralysis. They attract special attention, and some clinicians (for example, Hoche[155] in a recent article) regard them as specifically hysterical symptoms which seem absolutely incomprehensible. But this is an obvious misunderstanding: they do not differ in any way from positive symptoms. Do not we, normal people, constantly repress some of our movements and words, i.e., do not we send inhibitory impulses to definite points of the cerebral hemispheres? As pointed out in our physiological introduction, in the laboratory we constantly elaborate,

along with conditioned positive stimuli, conditioned negative stimuli. In hypnosis by means of stimulating words we produce anaesthesia,[156] analgesia, general immobility or inability to move certain parts of the body, functional paralysis. A hysterical person often can and must be regarded, even in normal conditions, as being in a chronic state of hypnosis to a certain degree, since owing to the weakness of his cortex, ordinary stimuli become superpowerful and are accompanied by a diffused transmarginal inhibition, just as in the paradoxical phase of hypnosis observed in our animals. Therefore, besides the fixed inhibitory symptoms, which, like the positive ones, appear at the moment of violent nervous trauma, the same inhibitory symptoms may arise in a hysterical hypnotic as a result of suggestion or auto-suggestion. Any notion of an inhibitory effect evoked either by fear, interest or advantage, repeatedly concentrates and intensifies in the cortex and, owing to the emotivity of the hysterical person, just as in hypnosis the word of the hypnotist, provokes these symptoms and fixes them for a long time, until, finally, a stronger wave of excitation effaces these inhibitory points.

The same mechanism of auto-suggestion produces in a hysterical person a multitude of other symptoms, some of which are rather ordinary and frequent and some extraordinary and highly peculiar.

Any slight sensation of pain or the slightest anomaly in any organic function engenders in a hysterical person the fear of becoming seriously ill; and this suffices not only to maintain these sensations, again by means of the above-described mechanism, but to reinforce them and bring to such a pitch of intensity as to render the subject invalid. However, this time it is not the positive aspect of the sensation that is responsible for its frequent reproduction and predominant action in the cortex, as is the case in war hysteria, but, on the contrary, its negative aspect. This, naturally, makes no difference as regards the essence of the physiological process. Unquestioned cases of imaginary

pregnancy accompanied by corresponding changes in the mammary glands, by an accumulation of fat in the abdominal wall, etc., are examples of peculiar manifestations of hysterical auto-suggestion. This is further confirmation of what has been said in the physiological introduction to this article concerning the cortical representation not only of the activity of all organs, but of separate tissues. At the same time this testifies to the extreme emotivity of hysterical persons. It is true that in this case the maternal instinct, powerful in itself, reproduces by auto-suggestion such a complex and specific state of the organism as pregnancy, at least certain of its components. The same mechanism is responsible for the states and stigmates of religious ecstatics. It is a historical fact that the Christian martyrs endured their tortures with patience, even with joy, and when dying, lauded those for whom they sacrificed themselves; this is striking proof of the power of auto-suggestion, i.e., of the strength of concentrated excitation in a definite cortical region, excitation accompanied by a very intense inhibition of all other parts of the cortex representing, so to speak, the fundamental interests of the entire organism, its integrity, its existence. If the power of suggestion and auto-suggestion is so great that even the destruction of the organism can take place without the slightest physiological resistance on its part, then, in view of the already proved high ability of the cortex to influence the processes of the organism, it is easy to understand from the physiological point of view the partial violation of the organism's integrity produced by suggestion and auto-suggestion by means of trophic innervation, the existence of which has been also proved.

It is, therefore, impossible not to see the erroneousness of the extreme point of view put forward by Babinsky,[157] although in general he correctly appraises the fundamental mechanism of hysteria. In his view the only symptom that should be regarded as hysterical, is the one provoked or eliminated by suggestion. This conclusion overlooks the

extreme intensity and incessant action of the given emotivity, which cannot be produced in a full measure deliberately by suggestion, especially since the real cause and nature of this emotivity may remain unrevealed.

Finally, it is necessary to touch on the fantasticism of hysterical persons, on their detachment from reality and frequent twilight states. It can be assumed that these symptoms are interconnected. As shown by the observations made by Bernheim and others on hypnotized normal subjects, as well as by our observations on dogs mentioned in the physiological part of this article, we must distinguish in hypnotism a number of gradations, beginning with a state, which hardly differs from wakefulness and ending with complete sleep.

In order to embrace and fully understand all the degrees of hypnosis, especially in man, I think it is necessary to dwell on the following problems, which have not only been insufficiently elaborated by science, but are not even properly formulated.

Life clearly reveals two groups of human beings: artists and thinkers. There is a striking difference between them. The first group, artists of all kinds—writers, musicians, painters, etc., perceive reality as a single whole, i.e., the entire living reality without breaking it up or decomposing it. The other group, the thinkers, on the contrary, dismember it, thereby, as it were, killing it and making of it a kind of temporary skeleton; only afterwards do they gradually as if anew assemble its parts and try to revive it, but this, however, they are unable fully to accomplish. This difference is particularly manifest in the so-called eudetism of children.[158] I recall a case which greatly amazed me forty or fifty years ago. In a family of a marked artistic disposition the parents used to entertain their two or three years old child (and amuse themselves at the same time) by showing him a collection of twenty or thirty photos of different relatives, writers, actors, etc.—and simultaneously pronouncing their names. The effect was that the child memorized the photos

and then called all the persons represented on them by their proper names. But how great was the general surprise one day when it was discovered that the child could give the right names by looking even at the back of the photo. Apparently in this case the brain, the cerebral hemispheres, perceived the optic stimulations in exactly the same way as a photographic plate reacts to the fluctuations of the intensity of light or as a phonographic disc records the sounds. And this, perhaps, is the essential feature of any kind of artistic faculty. Generally, such an integral reproduction of reality is inaccessible to a thinker. That is why the combination in one and the same person of great artist and great thinker is an exceedingly rare phenomenon. In the overwhelming majority of cases they are represented by different individuals. Of course, in the mass there are intermediates.

I believe that there are definite physiological grounds, although as yet not very convincing, for interpreting the matter in the following way. In the artist the activity of the cerebral hemispheres, while developing throughout their entire mass, least of all involves the frontal lobes and concentrates mainly in other parts; in the thinker, on the contrary, it is most intense in the frontal lobes.

Repeating what I have just said, for the sake of systematization, I view the higher nervous activity as a whole like this. In higher animals, including man, the first system establishing complex correlations between the organism and the external environment is represented by the subcortex adjacent to the cerebral hemispheres with its highly complex unconditioned reflexes (in our terminology), or instincts, drives, affects, emotions (in the usual diverse terminology). These reflexes are produced by a relatively limited number of unconditioned external agents, or in other words, those which act right from the day of birth. Hence, a limited capacity of orientation in relation to the surrounding world and at the same time a low degree of adaptation. The second system is represented by the cere-

bral hemispheres, excluding, however, the frontal lobes. It is here that a new principle of activity arises with the help of conditioned connection or association—the signalization of a limited number of unconditioned external agents by a countless number of other agents, which at the same time are constantly subjected to analysis and synthesis and ensure a very wide orientation in relation to the same medium and thereby a much higher degree of adaptation. This is the only signalling system in the animal organism and the first signalling system in man. In the latter another system of signalization is added: it can be assumed that this system relates to the frontal lobes, which in animals are much less developed than in man. It represents a signalization of the first signalling system by means of speech and of its basis or basal component—kinesthetic stimulations of the speech organs. In this way a new principle of nervous activity arises—abstraction and at the same time generalization of the countless signals of the first signalling system which is again accompanied by analysis and synthesis of the new generalized signals—a principle which ensures unrestricted orientation in relation to the surrounding world and ensures the highest degree of adaptation, namely, science, both in the form of human universal empiricism and in specialized forms. This second system of signalization and its organ, representing the latest acquisition in the process of evolution, are bound to be most fragile and susceptible to diffused inhibition when it arises in the cerebral hemispheres at the initial stages of hypnosis. Then, instead of the activity of the second signalling system, usually predominant in the alert state, the activity of the first system comes to the fore, liberated from the regulating influence of the second system; at first it takes the more stable form of reverie and fantastic imagination and subsequently the more acute form of a twilight state or light sleep (corresponding to the intermediate state between sleep and wakefulness or to the state of falling asleep). Hence the chaotic character of this activity, which no longer reckons with

reality, or if it does, then only slightly, and is mainly dependent on the emotional influences of the sub-cortex.

From what has been said it will not be difficult to appreciate from the physiological point of view what the clinicians term disturbance of psychical synthesis in hysteria (the expression used by Pierre Janet) or the split "ego" (Raymond's expression). Instead of a co-ordinated and well-equilibrated activity of the three systems mentioned, in hysteria this activity is continually dissociated, and the natural and law-governed interdependence of the systems is deranged; meanwhile the interconnection and proper interdependence of the work of these systems constitute the foundation of a sane personality and underlie the integrity of our "ego."

In the final analysis, different combinations of the following three particular physiological phenomena are constantly manifest and make themselves felt against the fundamental background of cortical weakness in hysterical persons: quick susceptibility to varying degrees of hypnotic states due to the fact that even normal life stimuli are superpowerful and are accompanied by transmarginal diffused inhibition (the paradoxical phase); extreme fixation and concentration of the nervous processes in definite points of the cortex due to the predominance of the subcortex; and, finally, undue intensity and extensity of negative induction, i.e., of inhibition caused by low resistibility of the positive tone of other cortical parts.

In conclusion, I take the liberty of saying a few words about hysterical psychoses. A case of this kind of psychosis has been demonstrated to me; it is a case of hysterical puerilism[159] in a woman of more than forty, who became ill as a result of severe shocks experienced in family life. She was unexpectedly deserted by her husband who some time later also deprived her of her child. After an attack of stupor and a general prolonged paresis[160] the woman sank into dotage. At present she behaves like a child, without, however, manifesting any obvious general defects in

the intellectual and moral sphere or in personal life. A closer examination of the patient shows that everything seems to be accounted for exclusively by the absence of the analytical inhibition which always accompanies our behaviour, our movements, words and thoughts and which distinguishes the adult from the child. Does not the development of our personality consist in the fact that under the influence of education and religious, social and civic requirements, we gradually learn to inhibit, to repress that which is not admitted, which is prohibited by the factors just mentioned? Is not our behaviour in the family circle or in the company of friends quite different in all respects from that under other conditions? The universal experiments of life prove this beyond all doubt. Do we not constantly encounter the fact that in fits of passion, which overcome the cortical inhibition, men speak and act in a manner which they regard as inadmissible when they are calm? And do they not bitterly regret such behaviour when the fit of passion recedes? This is particularly evident in the state of alcohol intoxication when all brakes are abruptly switched off, as aptly expressed in the Russian proverb: to the drunkard the sea seems up to his knee.

Will this patient ever return to a normal state? Well, it depends. The psychiatrists affirm that in youth such a state persists only for hours or days, although it is sometimes more protracted. In the given case it is a state of relative calm and satisfaction; it is probably determined by the previously described nervous mechanism, which makes the patient take refuge in illness in order to escape the difficulties of life and owing to which this pathological state may in the end become irremediably habitual. On the other hand, the disturbed and overstrained inhibition may weaken and disappear altogether.

Is hysteria in general a curable disease from the physiological point of view? In this respect everything depends on the type of nervous system. It is true that the predominant and encouraging impression produced by our work on

conditioned reflexes in dogs is that the cerebral hemispheres offer great possibilities for their training, although naturally these possibilities are not unlimited. When dealing with an extremely weak type we can, in exceptional, so to speak, hot-house experimental conditions, obtain an improvement, a regularization of the animal's general conditioned reflex activity, and nothing more. A durable transformation of the type is, of course, out of the question. But since certain hysterical reactions of a general physiological character can also take place in more or less strong types as a result of powerful stimulations or violent shocks, a full return to the normal is, of course, possible in this case. However, the return can occur only if the series of shocks and excessive stimulations do not overstep their limits.

While it is impossible to read without keen interest the really brilliant pamphlet by Kretschmer on hysteria, in which the author reveals a strong and almost constant tendency to interpret the hysterical symptoms physiologically, Hoche's article in *Deutsche Medizinische Wochenschrift* in its January issue this year, makes a strange impression. Is it really the case that modern physiological knowledge does not throw any light on the mechanism of hysteria, that the clinic and physiology "have halted before hysteria as they would at closed doors?" The following reasoning in Hoche's article seems quite strange. Adhering to the view that analgesiae and paralyses constitute the fundamental feature of hysteria, he addresses the supporters of the theory of the pathogenic force of motives in hysteria with the question: why would the strong indignation felt by some of his listeners and readers in consequence of his adverse opinion of the above-mentioned theory, not render them insensitive to pain, if it were caused by a faradic current of high intensity? Then he cites other analogous cases: for example, why are the patients not cured by a similar method, i.e., by a strong desire to get rid of their illness, of their neuralgiae? In this connection I recall an instance from my stu-

dent days which deeply impressed me and all who witnessed it. A young woman was undergoing a plastic operation on her nose which had been dreadfully deformed by some disease. Right in the middle of the operation the woman, to everyone's surprise, suddenly made a calm remark in response to something said by the professor performing the operation. Evidently, the anaesthesia (which was general) had practically no effect. Yet the same woman attracted general attention by the fact that during the daily dressing of the post-operative wound she exhibited extreme sensitivity to pain. Clearly the strong desire to get rid of the deformity, probably intensified by sexual emotion, rendered the woman insensitive to the operation trauma and made her hope and believe that the surgical intervention would end in complete success. But after the operation, at any rate for a period immediately after it, when the coarse, strange-looking artificial nose bitterly and cruelly disappointed her, the same emotion, on the contrary, rendered her highly sensitive even to what was now carefully done to her nose.

Many cases of this kind are met in everyday life, as well as in history. When dealing with such cases, it is necessary to take into account: in strong and normal individuals the harmonious complex of strong emotions and of predominant cortical associations accompanied by an equally strong negative induction in all other parts of the cerebral hemispheres; in the weak nervous type—the hysterical mechanism described above.

FEELINGS OF POSSESSION (LES SENTIMENTS D'EMPRISE) AND THE ULTRA-PARADOXICAL PHASE[161]

(OPEN LETTER TO PROF. PIERRE JANET)

Would you deem it interesting to print this letter in your journal and at the same time express your views on the points made by me after careful study of the article published by you last year: "Emotions of the Persecution Delusion"?

I am a physiologist and of late, together with my colleagues, have devoted myself exclusively to study of the physiological and pathological work of the higher part of the central nervous system in higher animals (dogs), which corresponds to our higher nervous activity, usually called psychical activity. You are a neurologist, psychiatrist and psychologist. It seems that we should give proper consideration to our reciprocal work and co-operate in our research, for, after all, we are investigating the activity of one and the same organ (concerning which there can hardly be any doubt now).

The third part of your article attempts to interpret the feelings of possession. The basic phenomenon is that the patients objectivize their weakness, their imperfections, and attribute them to others. They want to be independent, but they are adamant in believing that other people regard them as slaves who are obliged to execute orders. They want to be respected, but it seems to them that they are

being insulted. They want to have their own secrets, but it appears to them that their secrets are constantly being disclosed. Like everybody else, they have their own intimate thoughts, but in their imagination these thoughts are being stolen from them. They have annoying habits or painful fits, but they ascribe them to other people.

You interpret this phenomenon in the following way. Many of the ordinary circumstances of life are very difficult, unbearable and painful for these patients. For instance, the presence at the dinner table of two ladies of the patient's acquaintance, towards whom she had never been ill-disposed before. This constant difficulty and the natural frequent failures fill the patients with anxiety and fear, and inspire in them the desire to get away from it all. Like children or savages, they attribute all their troubles to the malignant actions of others, and this signifies deliberate objectification. Further, you devote attention to the following detail: in all the cases cited by you, we have to do, in your terminology, with binary social acts: to be master or slave, give or steal, strive for solitude or seek company, etc. These contrasts are confused by the patients when they are in a state of depression, the disagreeable opposite usually bearing an objective character and relating to other people. For example, the patient passionately wants to be alone, locked up in her room, and actually she remains alone, but she is tortured by the thought that some malevolent person has contrived to get into the room and watch her.

One cannot but agree with all the foregoing, which represents an extremely interesting psychological analysis. But I take the liberty of disagreeing with you on the interpretation of the last point. You repeat more than once that, contrary to the general belief, these contrasts are not so easily distinguishable. You say: "*To tell and to be told* form a single whole and the one is not easily distinguished from the other, as is usually believed." And further: "*The act of insulting and the act of being insulted* are united by the general concept of insult; but the disorder shows that they

may be confused, that one may be mistaken for the other." You explain this confusion by a rather complex combination of feelings.

Availing myself of the facts established and systematized by you, I have resolved to take another way and to interpret them physiologically.

Our general notion (category) of contraposition is one of the fundamental and indispensable general notions, which, along with all others, facilitates and controls normal thinking and even makes it possible. Our attitude towards the surrounding world, social environment included, as well as towards ourselves, would be distorted to a very great degree if there were constant confusion of opposites: I and not I; mine and yours; I am simultaneously alone and in company; I offend and I am offended, etc. Consequently, there must be a profound reason for the disappearance or weakening of this general notion, and, in my opinion, this reason can and must be sought in the fundamental laws of nervous activity. I think that in present-day physiology there are definite indications to this effect.

In the course of our study of the higher nervous activity by the method of conditioned reflexes we observed and investigated in our experimental animals the following precise facts. In different states of depression, inhibition (more often in various hypnotic states) the equalization, paradoxical and ultra-paradoxical phases are manifest. This signifies that the cortical nervous cells, instead of normally producing (within certain limits) effects proportional to the intensity of the stimulating agents, in states of various inhibition, begin to produce effects either of equal strength, or inversely proportional to the intensity of the stimulus, and even of an entirely opposite character; this means that the inhibitory stimuli produce a positive effect, and the positive stimuli a negative effect. I make so bold as to suppose that it is precisely this ultra-paradoxical phase which causes the weakening of the notion of contraposition in our patients.

All the conditions necessary for the development of an ultra-paradoxical state in the cortical cells of our patients, are in evidence and have been clearly established by you. When these patients, being of weak constitution, come up against a multitude of life situations, they easily fall into a state of depression, anxiety and fear; they can, however, still desire or not desire something, they have their emotionally-reinforced and possibly concentrated ideas of what is desirable or undesirable (I am the master, not the slave; I want to be alone and not in company; I want to have secrets, etc.). And in such conditions this is sufficient to evoke in a fatal way an opposite idea (I am a slave; there is always somebody near me; all my secrets are being disclosed, etc.).

The physiological explanation of this phenomenon would be as follows. Let us suppose that a definite frequency of the metronome acts as a conditioned alimentary positive stimulus, since its application is accompanied by feeding and, because of this, evokes an alimentary reaction. Another frequency of the metronome acts as a negative stimulus, since it is not reinforced by feeding and produces, therefore, a negative reaction: the animal turns away when it is applied. The frequencies of the metronome beats constitute a physiological pair, the components of which, being opposites, are associated and at the same time reciprocally induced, i.e., one frequency stimulates and reinforces the action of the other. This is an exact physiological fact. Further, if a positive frequency acts on a cell which for some reason or other is in a weak state (or in a hypnotic state), then this frequency, according to the law of maximum, which is also a strictly established fact, inhibits the cell. This inhibition, in conformity with the law of reciprocal induction, conditions a state of excitation instead of inhibition in the other component of the associated couple. That is why the stimulus related to the latter now provokes excitation, not inhibition.

This is the mechanism of negativism or contralism.

If food is offered to a dog when it is in a state of inhibition (or hypnosis), i.e., when you induce it to positive activity—to the act of eating—it turns away and rejects the food. But when the food is moved away, i.e., when you give the dog a negative impulse aimed at inhibiting the corresponding activity, at discontinuing the act of eating, the dog, on the contrary, begins to reach for the food.

Evidently this law of reciprocal induction of opposite actions must also be applied to contrary ideas, which, naturally, are connected with definite (verbal) cells and also constitute an associated pair. Due to a state of depression or inhibition (in our experiments any difficulty arising in the higher nervous activity is usually reflected by inhibition), more or less intense stimulation of one idea leads to its inhibition and, by means of the same mechanism, induces the opposite idea.

It is easy to see that this explanation naturally embraces the peculiar symptom of the schizophrenics—ambivalency[162]—which arises under a highly extended and profound ultra-paradoxical state.

Many people, even scientifically-minded people, are moved almost to the point of anger by the attempts to give a physiological interpretation of psychical phenomena; they retort that such explanations are "mechanical," since they want to stress as strongly as they can the obvious inaptitude and absurdity of trying to link subjective feelings and mechanics. In my view this is an obvious misunderstanding.

At present, of course, there can be no talk of representing our psychical phenomena *mechanically, in the full sense of the word.* We are also far from being able to do this with regard to all physiological manifestations; the same thing applies, although in lesser degree, to chemical phenomena, and it applies fully to physical phenomena. A truly mechanical interpretation is still the goal of natural-science research; the study of reality as a whole, including ourselves, is advancing very slowly towards this goal, and much time will be required before it is reached. Modern

natural science as a whole is but a series of many *stages of approximation* to this mechanical interpretation, stages linked throughout by the supreme principle of causality or determinism, according to which there is no action without cause.

And if possibilities are now opening up for explaining the so-called psychical phenomena physiologically, they can be regarded as a certain, slight, very slight degree of approximation towards a mechanical interpretation. It seems to me that in many cases these possibilities are opening up.

Being now at the psychological stage of your research, you are interpreting the feelings of possession, establishing the conditions under which they arise, reducing them to their elementary components and, in this way, elucidating their general structure, i.e., you are also dealing with their mechanics, with their general structure, but in your own way. I, in the physiological stage of my research, am trying to bring our common problem a bit nearer to true general mechanics, interpreting your fact concerning confusion of opposite ideas, as the specific interaction of elementary physiological phenomena—nervous excitation and inhibition. In their turn chemistry, and, finally, physics, will further disclose these phenomena and their mechanism, thus steadily approaching the solution of our problem.

XII

FRAGMENTS OF STATEMENTS AT THE "WEDNESDAY" GATHERINGS

STRUGGLE OF I. P. PAVLOV AGAINST IDEALISTS

[EXPERIMENTS WITH ANTHROPOIDS. CRITICISM OF THE CONCEPTS OF YERKES[163] AND KOEHLER]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY" HELD ON MAY 16, 1934

Academician *I. P. Pavlov*: Here we have before us "Raphael"[164] the chimpanzee. You say to him: "Work," and he takes a seat in a definite place, near a big rectangular box. The box has sliding lids with openings of different shape—circular, quadrangular and triangular. The lower part has a small door and through this we place food which rouses "Raphael's" interest. Near the box we put fifteen or twenty sticks with sections of different shape—round, quadrangular and triangular. We place the food into the lower part of the box, with "Raphael" looking on, and then we close it. The box is of a special construction: it is necessary to insert a corresponding stick into the opening in the lid, and press heavily on it; only then the door in the lower part opens, and "Raphael" is able to take the food. This is what we call his work, and his work lasts for a very long time, for two, three months and even more.

Thus, the food is placed in the box in the presence of the ape in order to rouse his interest, and a bundle of sticks is placed alongside the box, some of them circular, some

tetrahedral, and some trihedral. At present "Raphael" has brought his work to a high degree of perfection.

For example, when a lid with a quadrangular opening is placed on the box, "Raphael" takes a tetrahedral stick and opens the box.

When we complicate the job by placing among many sticks only one with a square section, he makes an error and picks up a triangular stick instead of the square one. He makes the same error three times and then picks up the square stick and gets the food. We repeat the experiment. This time "Raphael" makes the same error only twice, and then takes the right stick. After several trials and several errors, in the course of subsequent experiments, he insists on taking the square stick only, no matter how deeply it is concealed in the bundle. Thus you see that "Raphael" errs, but in a uniform manner. There are round and triangular sticks in front of him, but he never takes a round one.

We then place on the box a lid with a round opening. "Raphael's" choice is excellent: he immediately finds the right stick, even if it is hidden in the bundle.

We change the lid once more. This time it has a triangular, not a round opening. At the first go "Raphael" confuses it with the square opening, which means that as yet he cannot differentiate precisely between angular forms; he takes a quadrangular stick, tries it and, since it does not fit, throws it away. But he does not repeat this error; he finds the triangular stick no matter how it is hidden among the others. The following detail should be added. Here I shall have recourse to a certain degree of imagination, although in a way which seems to me absolutely legitimate. "Raphael" is a very gluttonous fellow, indeed; he accomplishes the entire procedure only if he is well recompensed. As a rule, he shows no inclination to waste time on trifles. Along with "Raphael," there is "Rosa," a female ape, who, unlike him, is more disposed to exercise her intelligence than to satisfy her tummy. When offered food, she often pushes it away. We can say, therefore, that if she displayed

any interest in these exercises she would in all probability solve the task merely for the sake of curiosity.

In telling you all this I wish to stress the following. The activity of this ape is in no way inferior to that described with such great satisfaction by Mr. Yerkes and Mr. Koehler. They regard this activity as a manifestation of the special intelligence of apes and sharply distinguish it from the activity of dogs which they regard as an associative process. But what reasons did they have for this viewpoint? What is the difference between dog and ape in this respect? I would even ask what difference is there between this activity and the activity of a child? And again, in what way does this activity differ from that of the dog?

The fundamental difference is that the lower extremities of the ape can perform functions similar to those performed by the upper extremities. Consequently, the ape accomplishes his task with greater ease, that is, finds the right sticks, chooses one, and inserts it in the opening, etc. "Raphael's" success, is due, above all, to the highly-developed mechanical possibilities of his body compared with those of the dog; the dog has no hands, and lacks the mobile extremities provided with five separate digits which make it possible to choose, to seize, to place, etc. This means that the motor apparatus of the ape is much more perfect than that of the dog.

And then? Then the spectator is impressed by the fact that the apes greatly resemble human beings—hands and general behaviour. However, were we to trace the entire path traversed by "Raphael" before he attained his complex equilibration with the surrounding world in conformity with his sense organs, we would be able to say that, wherever we were successful in observing his behaviour step by step, we found nothing, absolutely nothing, that had not already been studied by us on dogs. This is a process of association followed by a process of analysis effected with the help of analysers and accompanied by an inhibitory process which facilitates differentiation and rejection of that

which does not correspond to the given conditions. Nothing more than this was observed by us in the course of our experiments. Consequently, there are no grounds for affirming that apes have some kind of "intelligence" which brings them closer to man, while dogs have not and are capable only of an associative process. I have gone back to the grudge that I had against certain psychologists. At first I renounced them, then I became reconciled with them to a degree, but now the facts have turned me against them once more. They, apparently, want their subject to remain unexplained. How strange, indeed! How they love the mysterious. Everything that can be explained physiologically they reject. But all the facts mentioned here took place before our eyes. "Raphael" analysed the actions which he had to perform with the box gradually and for quite a long time. He began by discerning the visual images of the sticks lying horizontally on the floor; he distinguished the angular, trihedral stick from the flat and tetrahedral one, as well as from the round one. When he had to choose the right stick he began with what I call a chaotic reaction. It seems to me I have already said that if we wish strictly to adhere to objective terminology, we must replace the American term "trial and error"[165] by the term "chaotic reaction." There is a nuance of subjectivity in the first term. But from the objective point of view this is a chaotic reaction. Take, for example, infusoria which move hither and thither in their medium in pursuit of definite aims—food, favourable conditions, better temperature, more appropriate composition, oxygen, and who knows what else; suddenly one of the infusoria gets caught in a certain noxious substance—in a cold or hot current; it begins to zigzag back and forth, and from side to side until it finds a suitable medium. This is termed the "method of trial and error." But I would prefer to call it "chaotic reaction," especially since every child begins with chaotic reaction.

"Raphael's" connection with the stick as an instrument of action was probably established long ago. He takes the

stick, and this is quite comprehensible, all the more so since it was previously inserted in the opening in his presence; consequently, we have here the action of imitative excitation. He tries the stick but it does not fit; since his action is not reinforced by food he discards the stick and picks up another one which is also thrown away; however, he now begins to distinguish between the sticks. After several trials he no longer takes the wrong stick; this means that extinctive inhibition has been elaborated to them. The third trial is crowned with success; he gets the food, and thus the action is reinforced. After several repetitions a connection is formed between the visual image of the stick and the success of the action. But then the lid of the box is changed; "Raphael" adheres to the stick which has already brought him success for several times. But this time its use is not reinforced with food; he is able to differentiate, rejects the stick and looks for another one in the same way, etc. Consequently, everything begins with the formation of an association, with analysis of the shape of the sticks. In the subsequent experiments he selects the sticks at random, since he does not relate them to the opening in the lid; but whenever the stick does not fit, he throws it aside, and thus extinction manifests itself. He tries another stick, and if it does not fit either, it too is discarded, and finally he finds the proper one. Thus he easily distinguishes one stick from another, but that is not sufficient to solve the problem. So far "Raphael" only analyses the visual images of the sticks, but does not connect them with the opening. Then the second phase begins: the formation of a connection between the visual images of the sticks and the shape of the opening. Obviously "Raphael" cannot establish the connection between the shape of the sticks and the shape of the openings for a long time, because he does not see the shape of the section of the stick, while he clearly sees the lid opening, which is either round, square or triangular.

Further, an association must be formed between the opening and the visual images of the sticks. When one of these

associations proves to be correct, when it is reinforced by food, "Raphael" begins to establish a connection between the visual stimulation produced by the opening and the visual images of the sticks, i.e., he begins to analyse. At one stage he distinguishes only a round opening from angular ones, but he still confuses different angular sticks. This means that the analysis must be further perfected. "Raphael" finally learns to distinguish the sticks precisely, and then the problem is fully accomplished.

Consequently, in this task there is simply constant association between the opening and the stick. There you have the whole of "Raphael's" man-like activity; his entire behaviour is based on analysis and association.

M. A. Ussievich: A dog with which I am working, upon being placed in the stand for the first time and seeing a revolving food receptacle, immediately began to turn it with his paw.

I. P. Pavlov: That is in complete accord with what I have said. The tendency to draw a psychological difference between ape and dog based on the process of association is nothing more than the secret desire of the psychologists to evade a clear solution of the problem and to render it mysterious and extraordinary. In this pernicious, I should say, disgusting tendency to depart from the truth, psychologists like Yerkes or Koehler fall back on such barren notions as, for example, that the ape went away, "meditated at leisure" in a human-like way, "has found a solution," etc. Of course, this is absolute nonsense, child's play, most unbecoming. We know very well that a dog often tries to accomplish a definite task but is unable to cope with it; however, it suffices to let the dog rest, say, for two days, and the task is successfully accomplished. Can we seriously say that the dog spent the two days considering the problem? Of course not. Simply fatigue has brought on inhibition, which confuses everything, renders difficult and destroys. This is a very ordinary phenomenon.

A long time ago someone told me—I think it was Speransky—that musicians studying a new melody often have a lot of trouble—nothing comes of their efforts. The more persistent they are the worse the result. In utter despair they abandon the effort. But after some time, when they resume work, all the obstacles are easily surmounted. This is merely accounted for by the fact that in the course of the study one becomes fatigued and the fatigue disguises the immediate result. But after a rest the ready result clearly manifests itself.

It is necessary to add that these facts can be explained without any difficulty. It is worth noting that when these experiments were performed one after another and in great number, "Raphael" erred much more frequently; he fell into a state of despair and picked up the sticks at random, just like a man when he is upset. This was an obvious manifestation of fatigue.

Then my attention was attracted by the following fact. When "Raphael" was unable to perform his task he did look away for a while, after which he turned again and accomplished the task. This fact can also be explained quite simply. When "Raphael" moved he had before his eyes the real images of the sticks; but when he turned away from these real impressions, he had before him only the persisting images of the traces of different sticks, and the association in this case was easily accomplished. This is perfectly natural. And that is how the matter should be actually interpreted.

Therefore, basing myself on the study of the apes, I affirm now that their somewhat complex behaviour is a combination of association and analysis, which I consider to be the foundation of the higher nervous activity. So far we have seen nothing else in their behaviour. The same can be said of our thinking. Beyond association there is nothing more in it.

THE NATURE OF INTELLIGENCE IN ANTHROPOIDS AND THE ERRONEOUS INTERPRETATION OF KOEHLER

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON SEPTEMBER 12, 1934

Academician *I. P. Pavlov*: . . . I have two incidental subjects: one about apes, the other about Mr. Sherrington. The apes are connected with Koehler. It would be more correct perhaps if I say: on the one hand, about Koehler, and, on the other, about Sherrington. I believe it will be more useful if I talk about Koehler first.

This summer I devoted some time to the study of apes. We began with experiments concerning the analytical ability of apes. But these data are not new and are of no great interest. During the last month we reproduced Koehler's experiments, for example, the superposition of boxes in order to take hold of suspended fruit, etc. Prior to this I had read very thoroughly, and as usual, not once but several times, Koehler's article "Investigation of the Intellect of Anthropoids."[166] Thus I was able to read about the experiments and to have the facts of the given experiments before my eyes. I must say that I am really amazed at the degree in which human minds can differ.

In my opinion, Koehler saw nothing of what was actually demonstrated by the apes. I say this without any exaggeration: he simply did not understand anything.

Koehler, as indicated by the very title of his article, tried to prove that apes are intelligent, that in this respect, unlike dogs, they are close to man. He even mentions a special experiment which showed that the dog, unlike the ape, is not intelligent, and the latter, therefore, is rightly called an anthropoid animal.

What proof does he advance?

His sole fundamental, but peculiar proof is this. When the ape is given the task of taking hold of fruit suspended at a certain height, and when for the purpose of accomplish-

ing it he needs definite instruments, for example, a stick and some boxes, all his unsuccessful efforts to get the fruit are not, according to Koehler, proof of intelligence. This is simply the method of trial and error. When the ape becomes tired, as a result of his unsuccessful efforts, he gives up and remains for some time in sitting posture. When he has rested he tries again and succeeds in accomplishing his task. According to Koehler, the ape's intelligence is proved by the fact that he sits for a period without doing anything. He literally says that, gentlemen. In his view the ape accomplishes some kind of intellectual work when it is sitting, and this proves its intelligence. How do you like it? It turns out that nothing but the silent inaction of the ape proves its intelligence!

And the fact that the ape uses a stick and places several boxes one on top of the other, this is not a manifestation of intelligence. When the ape acts, moves the boxes from one position to another, these are associations and not manifestations of intelligence; this is the method of trial and error. Koehler absolutely disregards these facts—all this is simply only association. But when the ape rests, when he is inactive, he performs certain intellectual work. Naturally, the only explanation one can offer for such reasoning is that Koehler is a confirmed animist, he simply cannot become reconciled to the fact that this soul can be grasped by hand, brought to the laboratory, and that the laws of its functioning can be ascertained on dogs. He does not want to admit this.

In reality the matter is quite different. For it is the processes disregarded by Koehler that are of greatest importance. I grasped and realized this while observing the behaviour of the ape. And I say that all this activity of the ape in trying now one, now another way of solving the task, is the intelligence, the reasoning in action, which you can see with your own eyes. This is a series of associations; some of them have been acquired in the past, others are formed before your eyes, and are either combined, united into a posi-

tive whole, or, on the contrary, are gradually inhibited and lead to failure. One can clearly observe the manifestation of some of the associations formed earlier in the ape, in the course of his life in the jungle, in his native surroundings.

It is clear that the ape is the perfect equilibrist, capable of maintaining his centre of gravity even in most incredible positions on a vertical support. When putting the boxes in position the ape first of all finds out in an empirical way whether they are firmly fixed. He places one box on top of the other, just as if they were stones or stumps, and then tests their firmness. He does not verify whether the surfaces of the boxes coincide; he simply gets up on them and begins to sway. In the event of failure, he rearranges the boxes in order better to adjust their parts, then jumps on to them again and tests their firmness once more. You are witnessing associations—associations acquired in the past and used by the ape as ready-made ones. These are the tactile, muscular, visual and other associations.

The ape continues piling up the boxes, depending on the height of the construction. It so happens that he picks up an extra box, climbs the pyramid and places the box on his head. This, you see, is an error, made in the process of elaborating the right association, the necessary connection.

One erroneous and very ancient association causes him great difficulty. He cannot overcome this association on the basis of reality.

He is given boxes of different dimensions; to attain stability it is necessary to place them in strict order, beginning with the largest one which must be placed at the bottom. But up to now he is unable to do this. If, for example, he mistakenly places, say, the sixth box instead of the second, there is no association which could tell him that this is inconvenient, that the wrong box must be removed; so he continues building. In this case only a lucky chance comes to his assistance. As for newly acquired associations, their successful elaboration depends exclusively on the exact order of the boxes. This is a visual association, and it is formed

before our eyes. The sight of a regular pyramid leads to success. This visual association favours success. The act of placing the boxes directly under the suspended fruit, is an association which already existed. Thus, we clearly witness the formation of our thinking, we see all the reefs it encountered on the way, and all its methods. This is actual intelligence, but Mr. Koehler disregards it: for him this is simply trial and error.

There are some essential details. If the ape is in a state of excessive alimentary excitation, his actions become very disorderly—he takes the boxes as they come to hand, for example, the sixth instead of the second, etc. External inhibition exerts a very great negative influence. All this is well known. It is only necessary properly to observe definite facts and to accord them their actual significance. Then everything becomes clear and unfolds before the eyes. Such is the entire activity of the ape. His thinking is clearly observed in his actions. And this is the proof of his intelligence. It shows that there is nothing in intelligence but correct or erroneous associations, proper or distorted combinations of associations. Koehler, however, maintains that it is not a matter of association. Meanwhile the entire intelligence consists precisely of associations. What distinguishes it from the development of a child, or from our inventions? For the ape the problem consists in getting the fruit without the help of a stick, and he does this before your eyes by the method of trial and error, i.e., by means of associations. It is absolutely clear. In what way does this differ from our scientific discoveries? It is exactly the same thing. Evidently, this is elementary intelligence, differing from ours only by the poverty of associations. The ape has associations which relate to the interaction of mechanical objects in nature. . . . Thinking once more about the reason for the ape's success compared with other animals, and why he is closer to man, we would say that it is precisely because he has hands, actually even four hands, i.e., more than we have. Because of this, the ape can enter into very complex relations with the surrounding

objects. And that is why a multitude of associations are formed in him, associations which do not exist in other animals. Since these motor associations must have their material substratum in the nervous system, in the brain, the cerebral hemispheres of the apes are more developed than those of other animals, this development being due to the diversity of their motor functions. We, humans, in addition to the diverse movements of our hands, possess a complex of speech movements. As is known, the ape is less capable of imitating speech than many other animals. Compared with him the parrot can have a greater stock of words. That is how I see the matter.

Koehler, evidently, is a victim of animism. Sherrington is another victim, but I will speak about this next time.

Such is Koehler's interpretation of the matter. That does not mean, of course, that he is not a highly intelligent man. These are quite different things. Quite a lot of intelligent people have also been animists.

I have had the opportunity of meeting Koehler. He is a most intelligent man, a man of vast erudition; he has profound knowledge, particularly in the domain of natural science. Will his intelligence enable him to overcome this animism? In his book he constantly refers to another volume to be written by him. I do not know whether it has appeared or not (*a voice*: "No, it hasn't"). Then I will venture the following supposition. His book, apparently, was written under animist influence, but subsequently he overcame his animism and in all probability he now takes a different view of the subject. That is why his second volume has not yet appeared.

Read Koehler's book and see for yourselves. To close one's eyes to the ape's activity, which is plain for all to see and the meaning of which is absolutely clear, is the height of absurdity, sheer nonsense. Koehler speculates that the ape goes into meditation when it is in a state of inactivity. But we observed this state time without number; it signifies mere extinction and nothing else.

Good-bye.

[CRITICISM OF SHERRINGTON'S IDEALISTIC CONCEPTS]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON SEPTEMBER 19, 1934

Academician *I. P. Pavlov*: I shall now turn to a criticism of Mr. Sherrington. I have taken my time over this, quite deliberately; I re-read his book several times so as to avoid any exaggeration, any superfluous statement or judgement. However, two weeks have passed and my opinion has not changed in the least.

There is no doubt that Sherrington is handling a subject with which he, obviously, is quite familiar—*The Brain and Its Mechanism.*[167] He has been a neurologist all his life, engaged in the study of the nervous system, though, more of its lower part, the spinal cord, than the higher part.

Comparing the laws of the brain and its mechanisms, he draws a very strange conclusion. It appears that up to now he is not at all sure whether the brain bears any relation to our mind. A neurologist who has spent his whole life studying the subject is still not sure whether the brain has anything to do with the mind. This is clearly expressed by him in the following words: "If nerve activity have relation to mind. . ." I did not trust my knowledge of English and so I requested others to translate it for me.

How can it be that at the present time a physiologist should doubt the relation between nervous activity and the mind? This is the result of a purely dualistic concept. This is the Cartesian viewpoint, according to which the brain is a piano, a passive instrument, while the soul is a musician extracting from this piano any melodies it likes. Obviously this is his viewpoint. Probably Sherrington is a dualist who resolutely divides his being in two halves: the sinful body and the eternal, immortal soul. I am all the more surprised that for some reason or other he regards knowledge of this soul as something pernicious and clearly expresses this point of view; according to him, if the best of us acquire

some knowledge of the nervous system this would be a most dangerous thing threatening the extinction of man on earth. He makes the following statement which appears to me rather strange: if man learns to know himself and on the basis of this knowledge to govern himself in an economical way (such economy is not bad since it means that he will preserve himself for a longer time), then our "planet will be re-liberated, free for the next era of animal domination." What do you think of that? What does it mean? Why, it's simply preposterous!

Very good, we will suppose that the relation of the soul to the body is similar to that of the pianist to the piano, but it still leaves us in the dark as to why knowledge of the soul may be pernicious. I would like to know how on earth it can lead to the extinction of man. Socrates counselled: "Know thyself." How, then, can a scientist, a neurologist, say: "Do not dare know thyself"? Strange as it may seem, Sherrington adheres to the motto proclaimed at one time by Dubois-Raymond, a man who was always ready to sacrifice truth to eloquence, to a witty phrase, and who said that the function of the brain should never be made known,—"ignorabimus."[168] Sherrington, it seems, gets pleasure from repeating the same words fifty years later. What does this signify?

"If nerve activity have relation to mind," then he is inclined to think that this concerns only inhibition. Thus, positive work is of no significance whatever, while inhibition, discontinuance of work seems to go very well with the soul. He literally says: "If nerve activity have relation to mind, we can hardly escape the inference that nerve inhibition must be a large factor in the working of the mind." Why is, then, the essential positive activity rejected as having no relation to intelligence, while inhibition is regarded as having such a relation? Gentlemen, can anyone of you, who has read Sherrington's booklet, say anything in defence of the author? I believe that this is not a matter of some kind of misunderstanding, thoughtlessness or misjudgement. I simply suppose that he is ill, although he is only seventy

years old, that these are distinct symptoms of old age, of senility.

Take my wife, for example. She is an obvious dualist. She is religious, but at the same time her attitude to things is not distorted.

How is it possible to come out against studying the problem, to affirm that this can lead to the end of mankind, to the triumph of the animal world? Indeed I would like to see this book translated by someone who reads English fluently. Why is such nonsense published, especially since Sherrington is regarded by many as an outstanding authority? I ask you to read it and, if you can, to say something in his defence. To me, it is strange in the highest measure.

Now I can prove to you that he is a dualist and animist. This is clear from the fact that in 1912, that is, twenty-two years ago, he said to me when we first met in London: "Your conditioned reflexes will hardly be popular in England, since they have a materialistic flavour." As I understand it, he obviously spoke for himself.

Here is another interesting passage. He puts the matter thus: "Strictly we have to regard the relation of mind to brain as still not merely unsolved but still devoid of a basis for its very beginning." He clearly states that we do not possess any starting-point, even the slightest one, for the solution of this problem.

Precisely this explains why towards the end of his life he has become a confirmed dualist and animist.

As for Descartes and his dualism, he, when he speaks of animals, regards them as genuine machines. This gave us the idea of reflexes on which we base our entire analysis of nervous activity. But when he speaks of man, Descartes is a dualist, believing in all seriousness that the brain is a piano, while the soul is the pianist, and that there is no direct connection between them. Thus, for Descartes' great intellect this was a complicated problem. He drew a distinct demarcation line between the animal and himself. In animals, the simple people used to say, there is only smoke or

vapour, while man has a soul. When I mentioned this in a conversation with Richet,[169] he, apparently anxious to uphold French thought, told me that this was not Descartes' real viewpoint, that the clergy had forced him to think in this way and to express such ideas, that in reality he obviously advocated our point of view.

Voice: There are indications that Descartes burnt his last book, an outstanding work written in an absolutely materialistic spirit, because he had a presentiment that he would be denounced by the church. This was his conclusive philosophical work.

I. P. Pavlov: I have never read anything about that. In those days, of course, this was no joke: They could easily burn him, do away with him. That is quite on the cards.

N. A. Podkopayev: There are indications that Descartes deliberately inserted corrections in his work to suit the censorship; there was a difference between his real views and what he wrote.

I. P. Pavlov: I have never heard of that.

Well, gentlemen, that is all. I recommend those of you who know English to read this book.

P. S. Kupalov: Of course Sherrington is a dualist. That is absolutely clear. It explains why the sense he generally imparts to certain words differs from yours.

I. P. Pavlov: But he literally writes: "If nerve activity have relation to mind."

P. S. Kupalov: What does he imply by the term mind?

I. P. Pavlov: He simply uses the term "mind."

P. S. Kupalov: But you interpret the concept mind in your way, while he regards it somewhat differently. He chiefly takes into account, so to speak, the subjective emotions. He agrees that behaviour is law-governed. But he is preoccupied with what we call—near sensations.

N. A. Podkopayev: That makes it all the worse.

I. P. Pavlov: Podkopayev is quite right when he says it makes it all the worse. What is meant by the statement that sensations have no relation to nervous activity? If by the

mind he implies not proper orientation in the surrounding world, but exclusively subjective emotions and subjective manifestations, for example, direct sensations, then it follows that sensations have no relation to the nervous activity.

P. S. Kupalov: At the beginning of his book he states that the mind is directed by the external world and that all human behaviour is conditioned by the laws of that world. So, as far as the first part of the book is concerned, there is no misunderstanding. But I don't quite comprehend the final part of the book. He puts the following question: if I start investigating the brain from a purely physiological point of view, will I discover anything other than the mechanisms of which I already know in the nerve cell? As he sees it, mind signifies rather spirit than intelligence.

N. A. Podkopayev: Such a formulation of the problem is, in itself, dualism. In order to throw a bridge over two things, it is necessary that they should greatly differ, that there must be a gulf between them.

I. P. Pavlov: He is right from the physiological point of view when he admits that the mind is a most delicate correlation between the organism and the surrounding world. I should like to ask you then, what remains for your subjective sensations? If all our relations with the surrounding world, even the most delicate ones, are nothing but our physiological brain, then there is no room for any other interpretation of the word "mind." And this is the kernel of the contradiction.

P. S. Kupalov: He expresses the following idea: if we thoroughly knew one another, to the very core, life would become absurd, stupid, impossible.

I. P. Pavlov: Your words reveal the torments of the thought expended in trying to solve this problem, but they lack clarity.

P. S. Kupalov: I see it in a different way. Let us take his very last phrase which begins with the words "May I be forgiven. . . ." and concerns the social type. . . .

I. P. Pavlov: That is really stupid; it has nothing to do with the question we are discussing and is further proof that we are dealing with an abnormal mind. The fact that you, his advocate, despite your attempt to comprehend his viewpoint, are baffled by his phrase, simply reinforces my position. He is an out and out dualist. I know other dualists, but they have not gone so far as to affirm that if you analyse this sort of thing, i.e., the mind, you will be threatened by annihilation and by animal domination on earth. His talk about liberating the planet from man is sheer nonsense. It means that we, who are crowning the evolution of living matter, are evil, a species of tyrants. It is from this point of view that his words about the planet which "will be liberated" can be understood and interpreted.

P. S. Kupalov: Liberated in order to be developed anew.

I. P. Pavlov: "Free for the next era of animal domination."

No, gentlemen, if you undertake to defend him you must at least fully understand what he writes.

P. S. Kupalov: He maintains that there is "animal domination" on earth at present, meaning the reign of the animal world, including man. Who governs the earth? It is man as the supreme representative of the animal world.

I. P. Pavlov: When he speaks of animal domination he implies not man, but animals, which are inferior to us.

E. A. Asratyan: Perhaps, this can be explained in a simpler way; perhaps, the social aspect of the problem is involved here too. Spengler[170] and other reactionary trends have tremendous influence in the West. It is rather an attitude towards science.

I. P. Pavlov: That simply shows the course of his abnormal thought. He speaks of this, obviously proceeding from a dualistic point of view, and then inappropriately quotes Spengler and others. He says that we need not be prophets to foresee the fast approaching extinction of man. But Spengler and others say something quite different, namely, that in view of the complexity of life, to which the human

system is not adapted, such excessive work may lead to the destruction of man. That is a different thing and has absolutely nothing to do with the study of the mind and of the nervous system. I can't understand it. In my opinion this phrase simply proves his abnormal thought. For it is a question of study, of scientific research, of economizing the nerves and intellectual forces, while Spengler speaks of the deformation of nervous activity as a result of excessive work which is beyond its strength.

E. A. Asratyan. I believe this stems from the same root.

I. P. Pavlov: As you like. But this is of definite interest. If these are torments of thought under the influence of dualism, then it is of general human importance and deserves to be given consideration. . . .

To solve particular, comparatively limited problems is one thing; but to tackle a problem involving the entire history of human intelligence is quite a different thing. We have just begun more or less to liberate ourselves from dualism. The human mind has for a long time been a prisoner of idealistic concepts. This should be borne in mind. And now it is time to end our discussion.

[CRITICISM OF THE GESTALT PSYCHOLOGY[171]]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON NOVEMBER 28, 1934

Academician *I. P. Pavlov*: Today our discussion will be devoted to psychology, or to be more precise, to the marriage of psychology and physiology. . . .

I shall devote myself specially to the gestaltists. . . .

First of all, what do the gestaltists represent? They are representatives, advocates, adherents of the idea of integrality. According to them, it is necessary to consider and to keep in mind the integral whole, the synthesis, the sys-

tem, and not isolated manifestations, which for some reason they dislike. Gestalt means design, pattern or image. The word is differently translated into different languages. For example, the English translate it as "form" or, even better, as the author calls it, "configuration." The English gestaltists are "configurationists." Gestalt is a German word and it has the same meaning. . . .

A few words about the radicalism of this psychology. I must tell you that it is quite young. It originated in 1912, and is but twenty-two years old. It represented a revolt against Wundt, i.e., against associationism—the system of psychology which dates from the 16th or 17th centuries and which to this day is, to a degree, predominant among psychologists.

"The Gestalt psychology revolted against analysis as the fundamental problem of psychology," as its principal task. This is a somewhat strange approach in view of the fact that all positive, modern science is fully based on analysis and inevitably begins with it.

We shall never arrive at any psychology if we do not analyse the human behaviour or experience.

Further, the Gestalt psychology has proclaimed that the notion of association is simply a misconception.

What a queer radicalism, indeed!

"Gestalt psychology was as opposed to the simple reflex as to the simple sensation." Such is their true radicalism! It cannot be expressed in a more distinct, in a more definite form. The gestaltists attacked Wundt and associationism solely because the latter defended the principle of analysis. Wundt stated that he first identified the elements and then worked up to larger and larger compounds, which is what science does in general. But the gestaltists refer to this approach as the "brick psychology"—a play upon words—or the "mortar psychology" crumbling everything with its pestle. Very nice, indeed!. . .

The author goes on to inform us that a still more important impulse to the development of this psychology was

given by the notion which appeared on the intellectual horizon in 1890 and which was introduced by a certain Ehrenfels, namely, the notion of "form quality."

This "form quality" means that elements remain elements, and are not worthy of any attention; of real importance only is the fact that from one and the same elements it is possible to obtain different wholes. The following example is cited. Take definite musical notes and compose from them different melodies. The melodies, of course, will be absolutely different. But this does not mean that the elements are of no value; it is thanks to them that the melodies can be formed and if they did not exist the melodies would be inconceivable. But this is no novelty! Why does the notion of form quality date only from 1890? Good gracious, how often do we meet the same in organic chemistry? Carbon, oxygen, hydrogen are elements which form carbon hydrates, acids, alcohols, etc. Where, then, is the novelty? And how can it be affirmed that the notion of form quality originated in 1890? Actually the idea is a very, very old one. But it made quite an impression on the psychologists. Woodworth even finds that it played the role of an impulse.

I must say that we have to deal with rather strange psychologists. At present I know them fairly well; I have also frequently met some of them. When I pointed out in my book the mosaic of the cerebral hemispheres, on the one hand, and the dynamic system, on the other, M. Piéron, a Paris psychologist, was greatly astonished and confused. Thereupon I wrote: let him open any page of any book on organic chemistry and consider any formula of chemical compound. He will see, on the one hand, a mosaic of hydrogen, oxygen, carbon, and on the other hand, their combination, the formation of a dynamic system. Do not all bodies represent dynamic systems?

Such is their thoughtlessness! They amuse themselves with a play on words, but disregard reality. This is absolutely clear.

Such, according to the author, is the origin of this "form quality"; it greatly attracted them and they chose it as their fundamental principle.

Since 1912 the Gestalt psychology has endeavoured to prove that any distinction in psychology between the elements and the whole is a misconception, that psychology invariably and exclusively deals with the study of the whole. But how can anyone get to know the whole without breaking it up? Take, for example, the simplest machine. How can the principle of its working be understood, if it is not dismantled, if the interdependence of its parts is not considered?

This is truly strange reasoning and it passes my comprehension.

The next chapter deals with the organic integrity of the psychical activity as the fundamental feature of the Gestalt psychology.

I must tell you that Woodworth endeavours to convey the ideas of others with astonishing thoroughness, I should say, even scrupulously. . . .

"We can certainly recognize in the Gestalt psychology a strong and valuable addition to the varieties of psychology." The gestaltists adhere to the point of view that an isolated feature should by no means be studied; they prove this in splendid fashion by stating that if a single feature of the face is exposed, and the rest of the face is covered, nothing will come out of it. This is self-evident. Separate features assume different importance in the entire whole; some of them stand out in bold relief, others are disguised, recede into the background, etc. That is quite obvious. But the features must be identified. In the end, when one analyses a face, one must on the basis of definite features depict it as quiet, calm, wilful, very tender, etc. Of course, without analysing the constituent parts one cannot comprehend anything. The same thing applies to the human character. If one takes isolated traits and analyses each of them separately, naturally it will be impossible to determine the given char-

acter; for this purpose it is necessary to take a system of traits and to establish which of the traits in this system are more prominent, which are hardly visible, etc.

The author adds that human and animal organisms are a "Gestalt." But nobody doubts their integrity. At the same time, however, nothing prevents the decomposition of this system into its component systems of blood circulation and digestion, as well as the decomposition of the latter into the stomach, intestines, gastric glands, etc. This is simply forcing an open door.

Further, it is said that our behaviour is not a mere sum of reflexes. Again, what a truth! That is a commonplace. But they picture the matter in such a way as if a system were a sort of a sack filled pell-mell with potatoes, apples, cucumbers, etc. Nobody has ever expressed such an idea. The moment one has to deal with an organism, it is clear that all its elements act one upon another, just as hydrogen, oxygen and carbon act in a chemical body depending on their location—from above, from the sides, from the right, from below, etc. This is a well-known, long-established fact. . . .

The gestaltists have given much attention to perception. What is perception? Some fifty or sixty years ago when I was studying in the seminary, and when there was not even a sign of the gestaltists, I learned from the same old professors and psychologists all about perception and what distinguished it from sensation, which is a more elementary process. The course of psychology at the seminary taught us that sensation is a kind of purer, so to speak, physiological stimulation, produced by a certain external agent on the sense organs; perception, however, is that which arises in the brain, when this stimulation is not single, but connected with other stimulations and old traces. It is this that enables us to get an idea of an external object. Such is perception. The final result of internal elaboration constitutes its very essence. So you see this is quite a commonplace fact, well known to everybody. . . .

But this has already been exhaustively elucidated from the physiological point of view. I am not acquainted with their works and their bibliography, and I don't know whether they made corresponding allusions or not; but they should have cited Helmholtz and his physiological experiments with the sensation of tones. Another of his classical books deals with the eye and the ear. There the explanations are not so vague; they are absolutely exact from the physiological point of view, from the point of view of the whole of the system. And so some fifty years prior to them everything had been explained physiologically.

These gentlemen should have made a proper study of physiology, i.e., they should have thoroughly read Helmholtz. But instead, they content themselves with a play on words: "the sensory brain process is ... subjected to the influence of the distance,"—but how—about this not a single word is said.

Perception, if considered profoundly, is simply a conditioned reflex; however, since Helmholtz knew nothing of conditioned reflexes he called them unconscious conclusions....

So you see that the gestaltists, far from contributing anything new to the problem, are not even aware of what was regarded as a truth more than fifty years ago. This is an indisputable fact. I defy the physiologists or psychologists to prove that I am not right.

Now let us turn to another point—to the gestaltist study of behaviour.

The following is literally stated: "Gestalt psychology dislikes the stimulus-response conception." What does this mean? These are scandalous words.

"It objects, first of all, to the idea that behaviour can properly be analysed into stimulus-response units." Thus they do not admit stimulations and reactions, i.e., they deny, for example, the fact that if I choke, it is because something is irritating my throat. They do not want to make any distinctions. But this won't get them very far! And what

do you think of this? "It objects to the notion of a bond between stimulus and response." This is literally stated. Read it and see for yourselves. They object to the notion, consequently, to the importance of a connection between stimuli and responses, whether provided by nature or inculcated by practice.

These are not my words; they are the words of the author.

I shall read one more passage, since it is a collection of magnificent absurdities.

They object to the theory that an instinct is simply a chain of reflexes; they object to the theory that learned behaviour consists of reflexes linked together by the process of conditioning. Moreover, they object to the loose way in which the term "stimulus" is used by psychologists. A psychologist speaks of a definite complex object as the stimulus. But they say: you have no grounds for affirming this, since various stimuli proceed from this object. But no one disputes that. For when I look at a certain object it may act simultaneously on my eyes and on my sense of smell if it has an odour. Why, then, is it impossible to use the term stimulus?

Now we pass, so to speak, to the pillars of Hercules, i.e., to the analysis of behaviour. Here we encounter another gestaltist. Apparently, gestaltists are recruited from among very superficial persons. Such, for instance, is Professor Kurt Lewin of Berlin University—don't trifle with this! Lewin has devoted himself to the study of the psychology of action. His arguments against the associationists, and in particular against the linkage between stimuli, do not go so far as to deny their existence in general. No, he does not go as far as that. But he affirms that the stimuli do not produce any action. This is also remarkable! "They are not the insufficient causes of action." He illustrates this point of view by splendid experiments—observations carried out on himself. All the experiments and observations of the teachers, and of the pupils, are cited in his book. So you can imagine what an intellectual beauty it is!

Suppose, for example, that I have put a letter in my pocket, impressing on myself the necessity of dropping it into the letter-box I pass in the street. In this way I have established a connection between the sight of the letter-box, as a stimulus and my response, my reaction, i.e., the necessity of placing the letter in the box. And when I see the letter-box I post the letter. The associationist or psychologist would cite this reaction as excellent confirmation of their doctrine. But he begins to argue.

"According to the association psychology the exercise of this stimulus-response connection should strengthen it." It is not bad, of course, that he is aware of reinforcement. "Therefore, when I reach the second letter-box my response of reaching in my pocket for the letter will be even stronger" (*laughter*).

Tell me, please, what does this mean? Is it not really absurd?. . .

Had he studied the matter more attentively, he would have said: I put the letter in my pocket. I had it with me, but being absorbed in my thoughts I forgot all about it and went past the letter-box. Later, I came to another letter-box which caught my eye; then my thoughts coincided, and I dropped the letter into the box. This is a true association. But he confused everything. The devil's own brew! Such are the gentlemen who analyse the higher psychological activity. They won't go very far!

[CRITICISM OF THE GESTALT PSYCHOLOGY]
(Continued)

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON DECEMBER 5, 1934

Academician *I. P. Pavlov*: Today we shall continue discussion of last Wednesday's subject. It merits consideration and is quite opportune since we are now trying hard to link up the psychological with the physiological.

First of all, I shall discuss in more detail what I mentioned in passing last time.

I have in mind a chapter in Woodworth's book devoted to Gestalt psychology. It is entitled "Insight Essential in Learning According to Gestalt Psychology." Learning, the concept of learning—such is the fundamental subject. I shall read a few excerpts.

"The trend of psychological theory, from the day of Ebbinghaus had been in the direction of a mechanical conception of learning."

And he adds: "On the other side, the work of Pavlov, and the enthusiasm with which psychologists took up the idea of the conditioned reflex, reinforced the old associationist conception of learning as consisting of linkages formed between . . . stimuli and responses."

"The Gestalt psychology is the chief opponent of associationism. It has no faith in these elementary linkages, whether native or acquired. Not that it dislikes brain mechanics—or dynamics; but it believes the brain to work in large patterns, by 'closing gaps' (I shall explain this later), etc., rather than by the operation of nerve paths linking this and that little centre in the brain."

What is meant by the phrase: "it believes the brain to work in large patterns, by 'closing gaps'?"

You will remember—I mentioned this last time—they took into consideration the fact that we distinguish cortical phenomena as a whole, and that if there is any hint indicating the existence of certain gaps, we fill the gaps ourselves. From this they have deduced a rather peculiar principle which they call "closing gaps." Koffka, one of the gestaltists, has written a book entitled *The Growth of the Mind*. Now, I ask those of you who are fluent in English, to tell me what the word "growth" means: does it mean increment or origin? According to the dictionary, it has both meanings. But there is a world of difference between them.[172]

Discussing the problem of learning, Koffka bases himself exclusively on the anthropoid studies of Koehler. He

comes to the conclusion that all learning consists in insight, and that Thorndike's supposed learning by trial and error is simply a mistake, one may even say, a misunderstanding. How do you like that?

Further he states: "Thorndike had pointed to the gradual improvement shown in his learning curves as evidence against any sudden insight."

Thorndike, just as we did in our experiments, kept his cats in cages; they learned to open the door, etc. Naturally, with the passage of time they performed this act more rapidly. And this is his learning curve. He finds that the fundamental learning curve, i.e., ability to open the door grows, improves quickly, and becomes more and more precise. That is why he says that there is no sudden insight, but gradual learning.

Koffka re-examined Thorndike's experiments and found that in some cases the solution was of a sudden nature. He cavilled at this point. Thorndike himself admits that many things, naturally, complicate the task. The learning and the final goal of learning can be attained at times more rapidly, at others in a more gradual way.

Further, Koffka gives his own exposition of the entire method employed by Thorndike and comes to the conclusion that "there is no learning except through insight." Insight, he says, does not simply exist alongside of trial and error as an additional mode of learning. The trial and error principle is merely displaced.

As Koffka sees it, Thorndike's trial and error principle means in the first place that nothing new can ever be learnt by the animal. The elimination of unsuccessful movements and fixation of the successful ones (according to Koffka) must go forward without any participation on the part of the animal. You see how preposterous his deduction is! The animal has not the slightest notion why its behaviour is being modified. The entire process, in which the successful acts are preserved and the unsuccessful ones gradually eliminated, is purely mechanical.

That is how Mr. Koffka puts the matter in his exposition of Thorndike's trial and error principle.

He seizes on an inexact expression of Thorndike's and traps him. In reality Thorndike says something altogether different:

"When put into the box the cat would show evident signs of discomfort and of an impulse to escape from confinement. It tries to squeeze through any opening; it claws and bites at the bars or wire; it thrusts its paws out through any opening and claws at everything it reaches; it continues its efforts when it strikes anything loose and shaky."

This is something quite different from what Koffka pictures, with the result that he wages a struggle not against the real Thorndike, but against an imaginary one, i.e., created by himself.

Such is the attitude of the gestaltists, as represented by Koffka, towards the object of learning.

Further, Woodworth has recourse to a proposition which they advanced with real success against associationism. I learned of this long ago from my correspondent in Paris, who acts as my liaison with Paris physiologists. There is much talk there to the effect that the gestaltists have adduced very serious and strong arguments against associationism. These arguments are based on the fact that conditioned reflexes are supposedly elaborated only to isolated stimuli, but not to relations between objects.

I reproduced this experiment jointly with S. V.[173] and we found that a conditioned reflex could be elaborated to a relation as successfully as to an isolated stimulus.

Their experiment consists in the following. They take two grey boxes, one of them having a darker shade than the other. Food is placed in one of the boxes, say, the one of a lighter shade. At first the animal confuses the two boxes; then, on the basis of the usual procedure of conditioned reflexes, it moves towards the box of lighter shade.

After that they take some other shades of grey, and the animal rushes towards the lightest of them, although this

is not the same stimulus that acted in the experiment with the first two boxes. Consequently, a reflex to a relation has been elaborated in the animal. And this, in their view, is a strong argument.

However, the substance of the experiment refutes their conclusions.

Together with S. V. Kleshchev we verified these facts on dogs. We elaborated a reflex to two tones with an interval of a quint. Then we began to differentiate other pairs of tones, some of them separated by a quint and some—by a third. It turned out that the pair of tones separated by a quint became differentiated more rapidly. Thus, a relation may of itself become a conditioned stimulus. There is nothing special in this. They, however, decided that on the basis of this experiment all the older theories of learning should be rejected and, consequently, that Thorndike's interpretation of his experiments should likewise be rejected.

The next section is devoted to the theory of learning itself.

I am compelled to enter the lists against the author.

The title of one of the paragraphs reads: "The Theory of Learning More Uncertain than Ever." I am most grateful to him. Because this means that he himself acknowledges his bankruptcy.

He affirms that there are now three theories: our theory of conditioned reflexes, Thorndike's theory, and the Gestalt theory. Each of these theories explains some of the facts; each is justified to a degree by corresponding experiments, but this justification is not sufficient to elucidate the questions raised by others.

I shall cite now his final conclusion: "We can certainly recognize in the Gestalt psychology a strong and valuable addition to the varieties of psychology. . . . There is probably profound truth in their contention that besides sensations and motor responses and bonds between them—besides and including all these—there is the process of "dynamic organization."

What do you think of it? Besides sensations, besides responses and bonds, there is a dynamic organization. This is a connection, and if not, then it is the soul which you consider, that is, something imperceptible, something you cannot feel with your hands. The connection is nothing else but this dynamic organization. I say that this idea of imperceptibility, of the soul, is ingrained in all of them.

I have told you what he says in his book.

Now for our interpretation of the question.

It must be assumed that the formation of temporary connections, i.e., of associations, as they are usually called, precisely represents this insight, knowledge, or the acquisition of new knowledge.

When a connection, or an association, is formed, this undoubtedly represents the knowledge of the matter, knowledge of definite relations existing in the external world; but when you make use of them the next time, this is what is called insight. In other words, it means utilization of knowledge, utilization of the acquired connections.

Consequently, gentlemen, the gestaltists start not at the beginning, but at the end. There are inborn connections; but when it is a question of a connection that is not inborn, it proves that if one thing follows another, you can establish such a connection. This is absolutely clear. All learning consists in the formation of temporary connections, and it is this which constitutes thought, thinking, knowledge. Thus, association, thinking, are the fundamental factors; this has long been known and has been properly appraised by some psychologists. The significance of the Gestalt psychology, which denies associationism, is wholly negative; it contains nothing positive.

I draw your attention to one idea to which the gestaltists stubbornly cling. In the concluding paragraph we find the following words:

"Where the existentialists are interested in sensory analysis, and the behaviourists in motor performances, the

Gestalt group stress the importance of the topic that has usually been called perception, neglected by the behaviourists and handled very meagrely by the existentialists." The whole of this sentence discloses absolute incomprehension of the matter. What does the word perception mean here? The connection between the kinesthetic stimulation of the cell and all other stimulations, etc. All these are perceptions, and all of them develop in the brain. It would be absurd to suspect Woodworth of believing that it is the contracting musculature itself which participates in the process of perception. Clearly, this takes place in the brain.

I am fully convinced that thinking is an association and I challenge anyone who disagrees with me to prove the contrary. Association is knowledge, it is thinking, and when you make use of it, it is insight. But beyond this there is considerable confusion.

The essence of the question is: how can the experimental forms of Thorndike be brought into line with ours? We apply the conditioned reflexes in a way that first a certain conditioned stimulus is put in action and then the unconditioned reflex is added. Thus, the stimulation produced by us is a signal of this unconditioned reflex. In the brain there takes place a prolongation of the path between the cells of the external excitation and the cells of the unconditioned reflex. That is our understanding of the matter.

Thorndike's experiments are quite different.

They are carried out in the following way. A cat is placed in a cage the door of which is bolted in a certain way. The cat tries to get out, prompted by the desire to be free, just like any other animal kept in confinement and whose movements are restricted, or possibly it is excited by the food placed outside the cage. How does it act? It makes numerous chaotic movements, and then, by chance, in the course of these movements, contacts the bolt; the cat acts on it mechanically also in an accidental way. Finally the door opens and the cat escapes.

It is clear that in this case a connection is formed between

the definite contact with the object and the mechanical pressure on it, say, the latch or bolt and the opening of the door. This is an association. The association consists in nothing but this, thus it is knowledge which the cat will use next time; this is comprehension of the relations between objects in the external world.

In the given case the cat was attracted by a piece of meat. But our ape "Rosa," who displays little interest in food, would do the same thing solely for the sake of escaping from the cage. In this case the connection is different. If, for the sake of getting a piece of meat, a dog or a cat succeeds in unbolting a door, it will act in the same way when satisfied, when it wants to be free. . . .

How should these facts be interpreted? It is necessary that at the given moment the brain should be in an active state, in a state of certain excitation. There is no doubt that the longing for freedom or for food is a reflex. It is a manifestation of the instinct. Take any animal, even the lowest, in which it is impossible to admit the slightest degree of intelligence, it will not turn away from food, on the contrary, it is attracted by it. And in exactly the same way it will not move towards any noxious agent, fire, for example. This is an unconditioned, inborn connection. When a dog reaches for meat or longs for freedom, this is an unconditioned reflex, an inborn instinctive connection. When the brain is in such an active state, an association is bound to arise, and this is intelligence, thinking, intellectual activity. At first it may be of a minimum degree, but subsequently it becomes more considerable owing to the formation of a connection. From this moment on thinking and comprehension make their appearance. Thus, it is association which underlies all these phenomena. It is in this way that our experiments must be compared with those of Thorndike. Precisely here lies the essence of the matter. In our experiments with artificial and alimentary conditioned reflexes, when connections are formed which act as alimentary signals and signals varying in accordance with

the conditions of the experiment, the connections are purely temporary, bearing the character of signals. In Thorndike's experiments the connections are of a more permanent nature. This is already the beginning of scientific knowledge, since it is a question of more constant connections. At the beginning they may be rather accidental, but science in general has at first a superficial character and subsequently becomes more and more profound, getting rid of everything accidental.

According to the mechanism of formation this is the same connection, the same association, but of different significance.

When you repeat one after another two words taken at random and having no definite meaning, one of the words finally begins to evoke the other. The mechanism responsible for the elaboration of this connection is the same—the formation of a path between definite cells. The gestaltists deny this. It follows that genuinely profound analysis is absolutely beyond them. In their eyes the matter is so complicated that one must not even touch it, must not try to analyse it. . . .

I shall pass now to our experiments on apes. Here it becomes even more clear that this "insight," this "thinking" (obviously one and the same thing) at first consists exclusively of elementary associations and then of connections between elementary associations, i.e., of complex associations.

We suspend a fruit at a certain height in "Raphael's" chamber. The connection established between him and the fruit is an unconditioned reflex, an instinct. He is attracted by the fruit, but the considerable distance between him and it is an obstacle. There are boxes on the floor. After a number of unsuccessful attempts "Raphael" soon notices the boxes. At first he stands on one of them and tries to reach the fruit, but the distance is still too great. Then he rejects this box as unsuitable and begins to pile one box on top of the other.

It must be granted that in this case the ape very likely proceeds from previous life experience. This is probably an old connection elaborated by life. . . .

As to the given association it can be assumed that either this experience existed previously, i.e., the ape already knew how to go about the matter, or being in a state of strong motor excitation, he seized one box, threw it accidentally on top of another and, standing on them, came nearer to the aim. Consequently, this is the same method of trial and error. Indeed, it is impossible to admit that the new connection was formed of itself, from things never seen by the ape before. In future we shall observe the actions of another ape right from the beginning.

The first association was thus established. In order to diminish the distance between himself and the bait, it was necessary to place one box upon another. The ape could do this in a firm and stable manner, or, on the contrary, he could place the upper box on the edge of the lower box. A truly useful association is obtained solely by the method of trial and error. Should the ape place the upper box on the edge of the lower, that is, without making their surfaces coincide, nothing would result. And this is another association. A connection must necessarily be formed in the head between the positions of both boxes. "Raphael's" complicated task consisted in piling six boxes one upon another in order to reach the fruit. Now he is able to accomplish this task. All these particular associations were elaborated by the method of trial and error. When the connection coincided with achievement of the aim, it was reinforced and fixed. In the final analysis, it is quite clear that different connections arise. This is clear to all.

In Koehler's experiments all the apes were kept together. In some of them the association was elaborated rapidly, in others—slowly, in still others it could not be formed at all; everything depended on the physiological properties of the brain.

Besides these constant connections, even if consisting of

various associations, one more important association is required: to place the box directly under the fruit.

When one of the apes reached the fruit, all the others saw it, and one of the most sluggish of his companions, directed by the imitation reflex, also piled the boxes one on top of the other, not, however, directly under the fruit, but a little to the side; thus, he made a fool of himself. When he climbed on to the boxes the apple was beyond his reach. Evidently this is the elaboration of isolated associations.

Along with the elaboration of such isolated associations, there must also be formed a chain of associations, linking one association with another. Thus, the entire mechanism of thinking consists in the elaboration of elementary associations and in the subsequent formation of chains of associations.

The importance of imitation must also be emphasized. One of Koehler's apes did not practise the method of trial and error—he simply watched the work of another ape. In this way he elaborated new connections, so to speak, at the expense of the work of another ape.

An amusing incident took place one day with "Rosa." "Rosa" is cleverer than "Raphael." She has a fairly high degree of "intelligence," while "Raphael" is simply a glutton. Food is the only thing that attracts him. With "Rosa," on the contrary, food is of minor importance. She often manifests an overwhelming desire to play, or even to "contrive"—to open a small box, etc. When she is busy with something or other, she rejects the food you offer her; the aim of her work is quite different. Unfortunately, this entails certain complications since the use of food is the simplest method.

In one of our experiments we utilized her playing instinct. We wanted to reproduce the experiment with boxes. For this purpose we constructed in a warm chamber a kind of a well. A limited space was surrounded by high walls. "Rosa" was brought in through a door. She wanted to play, but there was absolutely nothing in this space, except high

walls and a few boxes on the floor. And so the natural impulse of breaking loose came to the fore. She acted in a very curious and amusing way, just like Koehler's ape which, reproducing only part of the associations, made a fool of himself—he succeeded in constructing something but not in the right place. "Rosa" observed the door through which she had been let in. At first she simply tried to open it, but without success, since it was tightly bolted. She then discovered a tiny hole in the door, and availing herself of an old association, inserted her digit into it and made several attempts to force the door. However, these attempts, too, were in vain—the door was solid.

Then, picking up one of the boxes she carried it to the door, climbed on to it and putting her digit into the hole, began to tug at the door.

What is the significance of these actions? The significance is that in the large cage in which "Rosa" was kept, she frequently watched "Raphael" solve his problem. One of the elements of this solution was familiar to her, and she "thought" that it would help her to open the door. That was her aim, but she had seen "Raphael" reach his goal—the suspended apple—by carrying the boxes and piling them one on top of the other. This temporary connection was fixed in her, and she used it, but without success. Such is the explanation, such is the sole meaning. The ape performed this action once, and then repeated it. That is how I understand the matter.

This means that up to a point thinking is nothing else but associations; at first these are elementary associations connected with external objects, and subsequently they become chains of associations. Thus, each slight association, including the very first one, is the moment of the birth of thought. As I said at our previous gathering, these associations grow and increase in number. We then say that the thinking becomes more profound, more extensive, etc.

However, this is but half the process of thinking. It is what the philosophers, including Locke in his work on the

human mind, call synthesis.[174] And that is exactly what it is. It is, indeed, a union of impressions produced by two external objects and the subsequent utilization of this union.

But in addition to this association another process sets in—the process of analysis. As you know, the analysis is based on the analysing capacity of our receptors, and besides, on the disintegration of connections, which is also effected by the cerebral cortex. We know this process perfectly well from our experiments with the conditioned reflexes. If in experimenting with a dog you elaborate a temporary alimentary connection to a definite tone and then apply other tones without reinforcing them by food, the first thing observed is a temporary irradiation bringing the adjacent points to a state of excitation. We call it generalization. When the connection with the other tones is not justified by reality, the process of inhibition takes a hand. In this way the real connection becomes more and more precise.

The same thing applies to the process of scientific thought.

The habits of scientific thought consist, above all, in obtaining a more constant and precise connection and in the subsequent rejection of all accidental connections. From this point of view everything can be easily understood. The process of thinking begins with associations, with the synthesis; then the analysis joins in. The latter is based, on the one hand, on the analysing capacity of our receptors, of the peripheral endings, and, on the other hand, on the process of inhibition which develops in the cerebral cortex and sorts out that which does not correspond to reality from that which does. That is how I understand the matter, basing myself on the data of our research.

Gentlemen, if any of you would like to add to what I have said, or to amend any of the points, you are welcome!

From my point of view, the Gestalt psychology is one of the most unsuccessful essays of the psychologists. Its role, I should say, is definitely negative. Indeed, what has it

contributed to the knowledge of the subject? Absolutely nothing. On the contrary, it is destroying that which is most essential and most correct—associationism, synthesis, connection. Such is my attitude to this Gestalt psychology.

In any case, you should give thought to the matter, since it is of vital interest to us. All of us are studying the problem of the higher nervous activity, and you, our "conditionists," are taking part in solving it. I would recommend you, therefore, to concentrate on it, to consider all the pros and cons and to express your views, since this is the only way to establish the truth.

I think that the point of view which I have just expressed fully accords with reality. I cannot think otherwise now. . . .

If you have no points to raise at the moment, keep the subject in mind and think it over. It is a fundamental question. Psychology is covered here by physiology, the subjective is interpreted in a purely physiological, objective way. And this is a great gain. We are beginning to understand the process of human thinking, which has been the object of so much talk and of so much twaddle.

In any case, I am grateful to this book: it made me consider all these questions anew, in a more profound way and, in the end, enabled me to reach the conclusion I have laid before you.

[CONCERNING THE ARTISTIC AND THINKING HUMAN TYPES]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON JANUARY 9, 1935

Academician *I. P. Pavlov*: . . . Now, gentlemen, let us turn to the following question. When we analysed nervous patients in the neurological clinic, I came to the conclusion that there are two specifically human neuroses—hysteria and psychasthenia; I related this conclusion to the fact that man offers two types of higher nervous activity, namely, the artistic type, consequently analogous and close to that of animals which also perceive the external world in the

form of impressions exclusively and directly by means of receptors, and the other, intellectual type which functions with the help of the second signalling system. Thus, the human brain is composed of the animal brain and of the purely human part relating to speech. It is this second signalling system which is beginning to prevail in man. It can be assumed that under certain unfavourable conditions, when the nervous system is weakened, this phylogenetic division of the brain takes place anew; then probably one individual will use predominantly the first signalling system, while the other will use predominantly the second signalling system. And it is this that divides men into artistic natures and purely intellectual abstract natures.

In unfavourable conditions when this divergence attains a high degree, a morbid manifestation of this complexity of the higher human nervous activity takes place in the form of, so to speak, exaggerated artists and exaggerated thinkers (pathology). I think that the former are related to hysterical persons, and the latter to psychasthenics. I have seen many neurotics. As to the unsuitability for life or inactivity of these patients, it must be said that the psychasthenics are particularly feeble compared with the hysterical persons. This is confirmed by facts. Many hysterical persons become "prominent public men" (for example, the American lady who, being a typical hysterical person, founded a new religion, amassed millions and became famous.[175] On the contrary, psychasthenics, who limit themselves exclusively to words, are in most cases unsuitable for life and absolutely helpless. Of course, there are also hysterical individuals whose activity becomes so chaotic that they, too, cannot find their proper place in life, and who are nuisance both to themselves and others.

I put myself the question: and what about our animals? The existence of psychasthenics among animals is excluded, since they do not possess the second signalling system. In the final analysis, all complex relations in man have passed into the second signalling system. Verbal and abstract

thinking has been elaborated in us. The second signalling system is the most constant and ancient regulator of human relations. But there is nothing of the kind in animals. Their entire higher nervous activity, with its supreme manifestations, is included in the first signalling system. In man the second signalling system acts on the first signalling system and upon the subcortex in two ways: in the first place, by inhibition which is greatly developed in it, and is absent or almost absent in the subcortex (and which, it can be assumed, is less developed in the first signalling system); in the second place, by its positive activity—by the law of induction. Since in man the activity is concentrated in the region of speech—in the second signalling system—its induction must act on the first signalling system and the subcortex.

Such relations cannot exist in animals. But they can assume this form when the inhibitory process in the first signalling system (which in animals stands above the subcortex) is weak. Since the first signalling system in animals is also the regulator of the subcortex, a relation essentially similar to that in hysterical persons may arise; if the inhibitory process in the first signalling system of the animal is weak, the subcortex falls into a state of violent excitation and its activity does not correspond to the action of external stimuli. Consequently, something analogous to hysteria may also take place in animals. While in man we meet with pressure of the second signalling system on the first signalling system and on the subcortex, in animals it is the first signalling system which exerts pressure on the subcortex. Essentially this is the same thing, but in the second case there is a single source of inhibition, while in the first case there is a double source (partly the positive system and partly the intense activity).

This thought occurred to me when observing one of the dogs in Koltushi; this was "Verny," a truly violent, impetuous dog, a typical watch-dog, which would not let anyone but his master approach him. He also exhibited a violent

alimentary reflex. For a long time we have not been able to obtain in him a more or less stable system of conditioned reflexes. This is similar to what was observed in castrated dogs by M. K.[176] No dependence on the intensity, no complete differentiation, and quite often manifestation of the ultra-paradoxical phase. The course of reflexes in the period of retardation, i.e., in the period of the isolated action of the conditioned stimulus, is also of definite interest. During the first five seconds the dog exhibits an abundant secretion of saliva, and during the subsequent five seconds—complete absence of salivation (zero). I am ready to affirm that this is a hysterical dog, in which the first signalling system regulating the nervous system and the energy of the subcortex is absolutely feeble. There is no correspondence here between the action of the signalling system and the emotional fund of the subcortex. This is proved by the fact that when we reinforced the inhibition in the first signalling system (by means of bromide), a certain order began to set in. The administration of a large dose of 6 grammes resulted in the elimination of the chaos to a very considerable degree.

And so it is possible to regard "Verny" as being a really hysterical dog, since he does not possess in any sufficient measure the vital regulator of the subcortical emotional fund.

[EXPERIMENTS ON APES AND CRITICISM OF KOEHLER'S CONCEPTS]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON JANUARY 9, 1935

Academician *I. P. Pavlov*: . . . Now I shall busy myself with Koehler and our apes. The point that Mr. Koehler fully disregarded is to us, on the contrary, of special importance. The way in which the ape makes acquaintance with the surrounding world is of no interest to him. He ignores this, while we concentrate on it. When the ape sits without doing

anything, he is, probably, resting, but not meditating, as Koehler has it. We see how "Raphael" makes expedient acquaintance with the surrounding medium. Under the influence of alimentary excitation he explores the conditions of the external environment.

"Raphael" was solving a rather complicated problem—the piling up of boxes of different size with the object of reaching suspended food. The dimensions of the boxes varied in the proportion of 1 to 16. The boxes had to be so placed as to form a kind of stable stairway. The structure was a fairly high one—three and a half metres. But "Raphael" solved this problem before our eyes. He realized that the surfaces of the boxes should coincide as much as possible, and that the boxes should not be placed on their edges or angles. He assembled them by the method of trial. The experiment lasted about two months. Now "Raphael" builds quite well. The structure must be erected at the place where the fruit is suspended. And "Raphael" builds directly under the suspended pear, placing all the boxes in the proper order: the first, then the second, etc. When the boxes are scattered, he reassembles them and places them in the required order. Can there be any doubt that this is similar to our thinking? Koehler, however, ignores this.

Carried away by our experimentation, we are now enlarging in every possible way "Raphael's" "naturalist researches"; our assistance takes the form simply of diminishing the adventitious elements, that is, in creating certain favourable conditions.

We experiment with a fire which bars the ape's way to the food. "Raphael" rapidly considered the situation; he burnt and licked himself at the first unsuccessful attempts. His method was quite clear: he made use of various solid objects, chips, nails, etc. When the food was placed in the centre of a circle formed by burning candles, he knocked the candles over or blew them out. Recently he has learnt to extinguish fire with the help of water. Here is how he did it.

A vessel containing water was placed in a box. A tap connected with the vessel was attached to the top of the front part of the box. Fruit was placed in this box and was visible through an opening in the front. Just below the opening there was another small oblong vessel resting on a support; spirit was poured into this vessel and a match applied to the wick. The flame prevented the ape from getting the fruit. "Raphael" had to extinguish it. He tried one way, then another, and still another. Finally he noticed the tap, seized it and turned it. Water poured from the tap—the opening was so arranged that the water flowed direct on the vessel containing the spirit. After one or two repetitions "Raphael" began immediately to turn the tap at the right moment. In this way we came to his aid. When he turned on the tap for the first time he did it not with the purpose of setting the water in motion. However, he subsequently related the action of water to the extinction of the flame. And when the tap ran dry he took a bottle with water and poured it on to the flame. Is not this convincing enough?

In this way we shall acquaint "Raphael" with the properties of different phenomena and with the relations between them. And he will be able to make use of them. Koehler, however, ignores this, although it is the essence of the matter. This is the genesis of our thinking, which enables us to function. What distinguishes "Raphael's" experience from our own experiments, when we try now one way, now another until, finally, we establish the proper connection. Is there any difference? In my opinion, there is none.

After reading about the intellect of anthropoids, and witnessing these experiments I cannot understand how a psychologist, a man who deals with the problems of thinking, can overlook all this, and at the same time advocate the nonsensical idea that when the ape stops working he meditates just as we do. Is not this a peculiar way of thinking, of treating the subject? Yet, that is the way things are, and that is the way they remain. For some reason, our phys-

iological interpretation of these phenomena does not concern the psychologists in the least.

Koehler's latest book appeared in 1933, under the title *Psychologische Probleme*.[177] I have not read all of it. Part one is entitled "Behaviourism," part two—"Psychology and Natural Science." Koehler delivers broadsides against behaviourism. He mentions in passing that the behaviourists have accepted our conditioned reflexes with great enthusiasm. He makes this reference to conditioned reflexes: "I presume that the research carried out by Pavlov and his school is known." Just a single line! Thus, although he knows of our experiments, he does not say a single word about them; on the contrary, he vilifies them in every possible way.

He violently attacks the behaviourists. He affirms that the latter are guided by two commandments: "In science one must never admit any phenomenal world!", that is, must not admit our own manifestations as subjective phenomena. And further: "In the nervous system one must never rely on any functions except reflexes and conditioned reflexes." I am not sure whether he deliberately exaggerates this or not. But here is something which concerns us: "Observers are unlikely to find the reflexes and conditioned reflexes closely related to the study of complex forms of behaviour of animals and men or worthy of attention."

How do you like that? This means that they are so remote from the study of the behaviour of animals and man that he can hardly them "in Betracht nehmen" or consider them to any degree "nächst liegender"—closely related.

What strange blindness to affirm that this is not "nächst liegend" and "nicht in Betracht nehmen," when everybody knows that all the habits, all the connections (omission in text of the stenographic record)....

..."But those who are firmly convinced (he implies the behaviourists and us) that the original theory of conditioned and acquired reflexes is the whole truth (we have never claimed this for the nervous system), have no grounds

whatever for observing natural behaviour. They will have to begin a new study; they have no other functional notions."

How absurd! How can a professor of Berlin University, not an old man living out his days but a young man in the prime of life, talk such nonsense!

Whereas every one of our experiments is aimed at enlarging our concepts, he thinks that our knowledge of reflexes precludes any aspiration for further knowledge. This is strange, something truly amazing! And yet he pretends to know our conditioned reflexes. What is one to make of it?

For some inexplicable reason our way of interpretation is regarded as being "conservative." But how is that? How can it be "conservative" if lots of people argue with us and show no desire to comprehend our viewpoint? They regard our views as something monstrous, something they cannot accept.

"On the other hand, these conservative ideas are upheld and protected by the adherents of Pavlov and by all behaviourists, since they limit observation." This means that nothing more is desired by us. How could he reach such an absurd conclusion? "All the reactions of the animal nervous system are reduced to a couple of reactive forms—to conditioned and unconditioned reflexes."

Such is his attitude towards our conditioned reflexes. Please, tell me what it all means. As for me, I don't understand it. I have learnt from F. P.[178] that the author lectures on psychology at the theological faculty of Berlin University. Evidently that is not a place where our point of view is likely to be accepted. And that, I think, is the sole explanation of his thoughtlessness.

There are even more amazing and still less comprehensible things. In the chapter "Psychologie und Naturwissenschaft" he regards the naturalist hypothesis as a working hypothesis only, but, at the same time, audacious. He begins with the statement that our subjective world and our emotions can and must be subjected to observation, but it is useful to systematize them, and then, on the basis of

physiological data, somehow to superimpose the system of our subjective emotions of this objective system of physiological facts concerning the physiology of the nervous system. That is quite correct. Our aim is to study the objective, purely physiological facts, whereas the aim of psychology, if only it is capable of considering and comprehending this subjective world, is to superimpose one system on the other, which is what we are trying to do. We explain the phenomena of our subjective world on the basis of our physiological data. But imagine, his system is exactly the same. For he says that he has every ground to observe our emotions, our subjective states, to systematize them, to superimpose them on a corresponding physiological system and to establish a connection between them. Apparently he is aware of our research, since the results have been published in foreign languages. Nevertheless, for him all this is but a working hypothesis, an audacious hypothesis, and he adds the following, which seems to be his criticism: "We only see how on the basis of general ideas it is possible to deduce the real system of personal emotions superimposed on the structural properties of corresponding cerebral processes." This, it appears, is his criticism. But with us this is a constant fact; we know plenty of subjective manifestations that can be connected with objective data. Even in conversation during a private visit to him I told him how I interpreted the fact cited by him about a dog kept in a cage but within sight of meat which was placed beyond the grille. When the meat was placed at a distance from the cage, the dog immediately found a way out of the cage and took the meat. But when the meat was placed near the cage and greatly excited him, he behaved stupidly, trying all the time to get the meat through the grille. This means that a strong stimulus apparently produced negative induction. But for Koehler that is but an audacious hypothesis. He concludes: "... The system of personal emotions superimposed on the structural properties of corresponding cerebral processes, which are of decisive importance for the

interpretation and observation of the behaviour" ... and adds: "doch solang bis jetzt nicht beobachtet worden," i.e., which so far, however, have not been observed. What does this mean? Please explain it to me. I simply cannot understand him. The only possible explanation is that the torments of animism, deeply rooted in him, make him inconsistent, slow-witted and contradictory. There can be no other reason. I have met any number of medical people who were simply incapable of realizing that the entire behaviour of a patient could be explained without recognizing the active and independent role of the inner world. They could not understand how it was possible to take into consideration exclusively the influence of external stimuli, their summation, etc. This, I think, is the sole explanation of his highly inconsistent conduct.

Gentlemen, I recommend those of you who know German to read this book and express your opinion. I can understand this only as the torments of an animist obliged to adopt a scientific point of view. The spirit of the times demands it of him, but he lacks the necessary inner resources. . . .

When I visited Koehler in Berlin I was astonished at the reluctance with which he acceded to my explanations of the behaviour of his dog: "Yes, yes," he murmured making an obvious effort.

There is no need, however, to look for examples so far away. I had a close friend, a psychiatrist, to whom I ardently demonstrated our principles. I used to visit him on Sundays after my laboratory work. This lasted for several years. However, up to his death he was convinced that we were committing a grave error, since we did not take the inner world of the dog into consideration. And this man was a psychiatrist who knew very well how our soul changes and breaks when the brain is disordered. Such is the force of a habitual point of view.

This can be explained only by the fact that in the present case a fierce struggle takes place against the deeply-rooted

prejudices of human thought in the form of dualism. It is an interesting book, don't fail to read it. It contains glaring contradiction and inconsistency. Many interesting things will come our way when our interpretation of the behaviour of apes is published.

[CRITICISM OF KOEHLER'S IDEALISTIC CONCEPTS]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON JANUARY 23, 1935

Academician *I. P. Pavlov*: ... Now, gentlemen, we shall pass from peaceful affairs, if we may say so, to matters of war, to Mr. Koehler. We are at war with him. This is a serious struggle against psychologists. Koehler is professor of psychology at Berlin University. A scientist of minor authority would hardly be elected to a chair in Berlin University; they respect hierarchy there. They look on Koehler as an outstanding psychologist. I have been in his laboratory which is located in Wilhelm's palace. Don't trifle with it!

When I read his *Psychologische Probleme* published in 1933, I was about to write an article on our experiments with apes. It was my intention to say something in the preface about the Gestalt psychology, and I even wrote on this subject.

Here it is:

"The most important and indisputable of the oldest acquisitions of psychology as a science is the establishment of the existence of connections between subjective phenomena—the association of words, the most obvious phenomena—and then the connection between thoughts, emotions and impulses to action. One cannot but be surprised, therefore, that nowadays this scientific merit of psychology is depreciated or greatly belittled by a new fashionable trend in psychology—the Gestalt psychology. The fact of associa-

tion, established by the psychologists, becomes all the more important since it fully coincides with the physiological fact of temporary connection, the formation of paths between different points of the cerebral cortex; thus, it represents a fundamental case, a moment of contact, or to be more precise, a synthesis, identification of the psychical with the somatic, of the subjective with the objective. This is an event of great significance in the history of human thought, on the horizon of the single and exact human knowledge. The attitude of the Gestalt psychology is an obvious misconception."

Such was my opinion when I read his book.

That which is correct in the book is as old as the hills. It is doubtful if the psychologists-associationists ever numbered in their ranks anyone who regarded the world of subjective phenomena with their endless interconnections as a sack filled with apples, cucumbers and potatoes which do not exert any action one upon another. The psychologists-associationists know very well that different combinations of three elements alone—oxygen, hydrogen and carbon—originate countless systems in the form of different substances, each with its peculiar properties. And the eduction of elements together with their diverse synthesis enable the chemist to penetrate deeper and deeper into the structure of our planet as a gigantic whole. The animal organism, ours included, is, in like manner, a closely interconnected whole. Is not its study made possible thanks first and foremost to its decomposition into larger or smaller units and to their subsequent intermittent composition? Why, then, should the product of the higher animal organism, the phenomena of our subjective world, be studied by other methods which exclude decomposition and analysis? Precisely for this reason the new trend in the Gestalt psychology, its violent opposition to associationism, is an obvious scientific error. The unmerited success of this psychology among modern psychologists is to be explained solely by the fact that dualism still makes itself felt in their midst; dualism

is manifested in the form of animism which admits the existence of a peculiar substance opposed to the rest of nature and therefore requiring special treatment on the part of researchers compared with material phenomena.

My following categorical statement also bears a direct relation to this subject: "There is but one way to a truly scientific comprehension of phenomena in psychology—the way of analysis."

That is my opinion of the Gestalt psychology. Thinking it over, it struck me as being a very harsh view, since the conclusion might be drawn from it that that which is old is true, while the new is worthless. So I resolved to re-read the book. As is my habit, I carefully read over and over again the chapter specially devoted to association.

And I must say that this chapter greatly perplexed me. In my view it reveals utter thoughtlessness and inconsistency. . . .

Undoubtedly, close contact has been established between our physiology of the higher nervous activity, in the form of the theory of conditioned reflexes, and psychology. We are studying one and the same problem. Of this there can be no doubt. But whereas from the factual point of view our concepts and notions are fully grounded, practically indisputable, theirs are not. I should like to attach great importance to this fact, clearly emphasizing that in some things physiology at present offers more truth than psychology, that is, if Koehler is to be regarded as a serious psychologist.

Koehler considers the entire problem in its historical aspect. He focuses attention on the fact that it is much more difficult to memorize a series of meaningless syllables than those which make sense. He is unable to deny this fact. It has been affirmed by competent psychologists whose authority is beyond all doubt. Unable to refute this fundamental fact, he switches to the factors which contribute to this association. There are many of them. Since connections are ready to hand, the association is either found at once or

fixed quite easily. Koehler bases all his objections on the fact that the existing connections contribute to the formation of the given connection. . . . But that goes without saying. The old connections constitute, according to Koehler, a gestalt, i.e., a system of organization.

Summing up we can say that wherever there is a solid organization, combination or a gestalt right from the outset, there is, naturally, a ready association. But where there is no proper organization from the outset, the association is absent; it must be elaborated.

Koehler then passes over to physiological notions. Generally he admits the formation of paths between two excited centres of the cortex. "This hypothesis may enable us to understand why the excitation, after some repetition, takes such a definite direction and thus augments the conductivity of the connected fibres. On the contrary, one cannot see—'Sieht man gar nicht'—why the stimulus takes this direction from the very first."

Why does it take this direction from the very first? What do you think of that?

It reminds me of a passage from *Nedorosl** where Prostakova begins to argue with the tailor; when the latter explains that he spent much time in mastering the art of tailoring, etc., she counters with the peremptory question: "Tell me, then, who taught the first tailor?"

What does this mean? How is it that an intelligent man, a professor of psychology, cannot grasp the matter, cannot comprehend it? His attitude differs in no way from the question "Who taught the first tailor?"

Can any of you, gentlemen, dispute this? How can one say that no coincidence is required, that the gestalt is formed of itself, at one stroke!

And here is another of his stunts.

He says that the idea of a path formed as a result of

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* Comedy by Fonvisin, 18th century Russian satirist.—*Tr.*

numerous repetitions is obsolete, that now there is a new hypothesis: when two centres somehow fuse, the tonus of one cell is communicated to the other, and this forms a Gestalt system, an organization—instead of two systems there is now one. But this means that the association forms the gestalt and not vice versa.

He, however, draws the conclusion: "The new concepts of Woodworth are invalid. Association as a special independent and theoretical notion loses all significance." What do you make of all this? Explain it if you can.

It is precisely the process of association that Koehler describes. The activity of two cells, previously detached, is fused into a single system because of a coincidence in time. Consequently, this is association. But according to Koehler, there is no association at all.

In my opinion this is complete misconception. I fail to see in it any sensible human thought, any impartiality and logic.

Further, he cites the example of meaningless syllables, which, when repeated one after another, are connected and memorized with great difficulty, while many other things are grasped in a flash and retained in the memory. But everything depends on the conditions, as well as on the old connections. Is there anything incomprehensible in it?

The next point directly concerns us and is, therefore, of special interest to me. I would ask you to look at it closely and to try to understand it.

"From our point of view conditioned reflexes would sound better than associations. However, I cannot regard this notion as being more fundamental than the notion of association. It can even be said that the so-called conditioned reflexes are simply particular cases of association."

But that is exactly the case; this is not something that can be said, but which must be said,—"since, evidently, the stimulus indirectly connected with the reflex reactions becomes a stimulus only when it begins to act in connection with an adequate stimulus evoking the same reflex

in a natural way. Thus, an association of two sensory processes takes place."

So far his point of view coincides with ours.

Further, we read: "This association can become so strong that in the final analysis the new stimulus might be fit only to follow the trace of the adequate sensory process, but not to evoke it." What does this mean? What do you think of this Egyptian enigma? And how are we to understand his words that the new stimulus is fit only to follow the trace of the adequate stimulus, but not to evoke it? Can you explain this physiologically or in any other way?

N. A. Podkopayev: Maybe he wanted to say that the conditioned stimulus does not wholly reproduce the picture evoked by the unconditioned stimulus, that its effect is somewhat weaker, and the reaction not so intense.

I. P. Pavlov: But he literally says: "nicht diese nachrufen." He speaks of our facts but in a way that it is impossible to comprehend him.

E. A. Asratyan: Perhaps he wants to say that an extraneous stimulus no longer evokes the orienting reaction, previously evoked by it, but a conditioned reflex.

I. P. Pavlov: He mentions a true reaction, conditioned by an adequate stimulus, and says that it follows the trace of the adequate stimulus, but does not evoke it.

E. A. Asratyan: Maybe it is a misprint. (*Laughter.*)

I. P. Pavlov: That's a pretty poor defence. This is truly astonishing!

Nevertheless, fundamentally the question is of definite importance. This is a real struggle between psychology and the physiology of the higher nervous activity.

I should like to see this book translated. We would then be able to circulate it widely and invite the psychologists to read it. Let them come here and defend one of their leading men. Is Zeliony here? (Voice: He is absent.) What a pity, I'd have given it to him.

E. A. Asratyan: The thing is really absurd.

I. P. Pavlov: Our task is a very definite one; we clearly see that due to association a system, an organization, or, in Koehler's terminology, a gestalt, arises. Consequently, it is the associations which form the gestalt and not the gestalt which forms the associations. The latter concept is absurd. Recall, for example, our delayed reflex. Is it not a gestalt, a system, in which one and the same stimulus first acts in an inhibitory way and afterwards positively? This is a gestalt, a system, and we know how it was formed. Then take our dynamic stereotype. We apply our stimuli in regular succession. They become connected and, as a result, a gestalt arises, a system, formed by us on the basis of associations. How, then, can anyone deny such an obvious fact?

[CONCERNING THE ANIMISM OF SHERRINGTON AND THE CONSERVATISM OF ENGLISH SCIENCE]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON FEBRUARY 6, 1935

Academician *I. P. Pavlov*: . . . Here is another interesting fact concerning the general significance and interpretation of our work. When the German edition of my lectures on the higher nervous activity appeared, a characteristic notice, written by one of Sherrington's adherents, appeared in the English magazine *Nature*. It begins with different compliments and then says that the correctness of the interpretation accorded this vast and grandiose material is open to doubt. For this reason, it continues, some people doubt whether the Pavlovian terminology can contribute to clear understanding of the matter. It is possible that in view of the present state of our knowledge it would be more advantageous to interpret these discoveries in psychological terms, such as association, distraction, interest, consciousness, attention, memory, etc.

What do you think of that? They themselves erect this structure, fully convinced that they are doing useful work.

Sherrington himself investigated the reflex activity of the spinal cord, but he is decidedly against attributing this activity to the higher parts, to the brain; in the latter case this structure becomes in their eyes hypothetical.

This is animist reasoning. Sherrington has built a nest of animism. This is proved by the fact that he doubts whether the mind has any relation to the nervous system. Hence the mind is something beyond and above the nervous system, something that can be detached from the nervous activity altogether.

I can understand the influence usually exerted by a teacher on his pupils. But must all the pupils necessarily be animists if their teacher is an animist? Is there really such intellectual serfdom among Englishmen? How are we to understand this reasoning? The man who makes it is one of Sherrington's adherents; he also cites his colleagues. And he affirms that it is better to systematize from the psychological rather than from the purely physiological point of view. This is all the more astonishing in view of the fact that the conditioned reflexes have won particular success in England, where they are even included in the programmes of the secondary schools.

In my view the stand taken by Sherrington is manifestly harmful, since he trains such disciples. He is at liberty to think as he pleases, but why should he confuse others?

No, we can confidently rely upon our conditioned reflexes.

Good-bye.

[CONCERNING THE IDEALISM OF PIERRE JANET]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON FEBRUARY 20, 1935

Academician *I. P. Pavlov*: ... I read now Pierre Janet's latest book *The Sources of the Intellect*. Pierre Janet is a remarkable man. He is not a physician, but a psychologist, and at the same time a celebrated neurologist. He is,

undoubtedly, an outstanding personality. I shall deal with the essence of his book next Wednesday. The book itself, in conception and analysis, is most interesting. I shall devote more time to it, since it deals with the highly important problem of correlations between the physiology of the higher nervous activity and psychology.

I am waging a violent war against Pierre Janet as a psychologist. And in my next talk I shall do all in my power to deal him a shattering blow. But as a neurologist he is of great interest. He has collected a considerable quantity of extremely interesting and important pathological cases. I am sure that as a neurologist he will always remain in the memory of science, but as a psychologist, I think, he will be discarded as a result of our work, the work of the physiologists of the higher nervous activity.

Janet describes two extremely interesting pathological cases of which the first is as follows.

It relates to a lady who was in a state of great exhaustion after the pains of childbirth. While travelling by train to her destination she was tormented by the thought that she was going in the opposite direction, although there were no grounds whatsoever for this, and her fellow-travellers confirmed that the train was going in the right direction.

What does this signify? This is a pathological phenomenon, a kind of obsession. It is one of the variants of the series mentioned earlier. Suppose, for example, that the patient wants to be respected, but he has the impression, without the slightest reason, that he is being insulted. Or he wants to be alone and secludes himself, but is convinced that there is someone in the room. As I have already explained, this is the ultra-paradoxical phase. And all these phenomena are categories of opposition. We have here a fundamental stimulus in the form of the idea: I am travelling in a definite direction; then comes the hypnotic phase: the monotony of stimuli acting in the car. Exhaustion of the nervous system as a result of a difficult delivery is also

in evidence. All this brings on the ultra-paradoxical phase and the rise of an opposite idea, or distortion of the fundamental idea. For example, the idea: I am alone, becomes reversed: I am not alone; the idea: I am respected or I want to be respected, turns into its opposite: I am not respected. The thought: I am going in the right direction, assumes a contrary meaning. In my open letter to Pierre Janet I gave my interpretation of this phenomenon. It is an old story and there is nothing special in it.

The second case interested me particularly.

It concerns a French officer who during the war was wounded in the occipital region of the brain. The bullet passed through the posterior cerebral part and became embedded in the opposite side. For some reason or other it was impossible to remove it.

The officer lost his sight. It was restored later only to be followed by the so-called "physical blindness." He could see, but could not understand; this is the so-called "Munk blindness." At a later stage he began to comprehend everything he saw, namely, that a man is a man, a table is a table, etc. Thereafter his visual comprehension became extremely concentrated, and the following occurred. I quote Pierre Janet: "The patient enters my consulting room leaning on the arm of a soldier, since he is convinced that he cannot walk without assistance. He recognizes me, greets me amiably and correctly, seats himself in an armchair and immediately begins to complain, unburdening himself of an "extraordinary grievance." This is what he said: "I am most miserable, because I have lost the ability to orientate myself in the world. I never know where I am." These are his exact words. They mean complete absence of orientation in space. . . .

This is an extremely interesting case, but how to interpret it? Basing myself on our observations I made two suppositions. This, apparently, is a matter of the occipital region, of the patient's visual relation to the surrounding world.

The same phenomena which we observe in our "Rebus" are manifest in his visual region: this region is inhibited to such a considerable degree that it cannot endure two simultaneous stimulations. You will recall that "Rebus" was unable to form more than one conditioned reflex—a stronger reflex destroyed the weaker one, for example, the defensive reflex destroyed the reflex to acid and the latter destroyed the alimentary reflex.

Consequently, the visual region of the brain has such a low tonus of excitation that it is able, influenced by the given stimulus, to concentrate its activity at one point only, while all other points remain as if they were non-existent. Hence, the patient sees a distinct person, a distinct object, but he is unable simultaneously to perceive anything else, since the notion of space escapes him. Everything is confined to the point which is stimulated at the given moment. There are no traces whatever and that is why the patient feels "lost in the world"....

The fact that there is a complete or almost complete absence of traces in this officer is very interesting. Due to a low cortical tonus, only the existing stimulations are effective in him; when he is subjected to certain stimulations, the inhibition spreads to the other parts of the analyser. The remainder disappears from his consciousness. And this is what gives him the feeling of being "lost in the world."

In the few minutes left to me I shall tell you something that may interest you. Next Wednesday I shall make a general attack upon Pierre Janet; today I shall confine myself to a brief statement about him.

Of course he is an animist, i.e., he believes in a specific substance which is not subject to any laws and which is unknowable. In his explanations he refers to Bergson,[179] a rather violent French philosopher.

He writes: Bergson has presented us with a very beautiful model in order to make us understand how nature could create such a miracle as the eye. To us the eye seems to be

extremely complicated, and we are inclined to think that in order to comprehend it we must accumulate fact upon fact and combine them in various ways. However, when I want to lift my arm, it is not necessary for me to analyse this or that organ, this or that nerve or muscle in order to have the desire to do something with its help. All that is needed is the desire to act and everything fits into place. The living substance longed for light, wanted to grasp light, and this desire took shape in the eye.

He literally states: "This desire took shape in the eye." "This is a creative force, a substance of great power."

And further: "We have lost much of this primitive power, but we still use some of it in our imagination." We use the negligible remnants in our imagination! Does he, then, resemble us? Is it possible to agree with him? Of course, not. According to him, the imagination is a particle of the creative force which has resulted in my eye!

[EXPERIMENTS WITH "RAPHAEL"]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON MARCH 6, 1935

Academician *I. P. Pavlov*: ... Now I should like to say a few words about our apes.

As you know, "Raphael" has added greatly to his knowledge of the surroundings. He has learned to open locks with the help of the corresponding tools. This is an old accomplishment, but he has become quite proficient in the art. He has learnt to appreciate the significance of the keyhole, to insert the key and to turn it. Now he does this quite easily. He has learnt to put out fire by means of water—his own "scientific invention." He is now able to construct a tower with the help of cubes, arranging them in the form of a staircase, and to climb it. This was not acquired at once; many difficulties had to be surmounted.

He has elaborated numerous more or less elementary associations. Now he has been given the more complicated task of forming an association of associations.

This involves opening the door with the key, entering the room, extinguishing the fire blocking the way to the landing; then he must get out of the room and build a tower on the landing in order to reach a suspended fruit. In this way he is called upon to effect an association of associations.

It is interesting to note that he usually performs all the operations without a hitch until he gets to the landing where he sprawls on the boxes and only after some time does he begin to build the tower. This repeats systematically. Obviously it is a difficult mental effort for him and greatly fatigues him. Hence, rest is necessary. This fact is absolutely clear.

We have known for quite a long time that our conditioned reflexes also represent nervous work. We know also that a dog which prior to castration responds perfectly to our complex system of conditioned stimuli, is unable to cope with the same system after castration. It needs rest.

Thus, you see that we are penetrating deeper and deeper into the higher nervous activity, and we are now dealing with its rather complex manifestations.

[CRITICISM OF CLAPARÈDE'S BOOK *THE GENESIS OF THE HYPOTHESIS*]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON MARCH 27, 1935

Academician *I. P. Pavlov*: ... Now, gentlemen, let us turn to the psychologists. They are, assuredly, experts at playing with words. But they fully disregard facts, and are an exceptional type of thinking people.

I have received from a very amiable psychologist a copy of a new book. The author, whom I have met on several oc-

casions, is the permanent secretary at all international psychological congresses. His name is Ed. Claparède—Geneva psychologist. He has sent me his book, *The Genesis of the Hypothesis*. I have read those parts which directly concern us. What a strange thing it is to use the word "mind" without having any idea as to what it actually signifies! How can I speak of the mind if I do not know its real meaning?

He begins with the words: "According to different authors, the essence of mind . . ." and then he enumerates the definitions of mind given by different authors.

One psychologist regards mind as the knowledge of the aim to be attained, another as ability to combine, a third as the power of abstraction, a fourth as ability to form a correct judgement—the latter being a particularly clever definition, etc. It is also defined as the formation of a general idea, as the faculty of analysing and synthesizing, of comprehending, inventing, making tools, utilizing experience, learning, giving the right answers from the point of view of the truth, predicting with high precision, knowledge of the relations between phenomena, and so on—there is no end to the definitions.

"If we wanted to come to common understanding about these definitions (not counting all others), we should never be able to do so; moreover, we should never begin the empirical study of the act of intelligence." This is interesting, but the author himself could not refrain from giving a new definition: "The concept of the new situation" seems to me to be essential to the definition of mind; if the situation or the problem which is to be solved were not new, there would be no question of mind, but of some other process: memory, habit, routine, repetition, etc., in brief, automatism. "Our definition is in close harmony with the general usage which opposes the mind to instinct and habit."

He then begins to expound his definition which for some reason or other he considers better than all others. It is really amazing: They heap up words and cannot agree on

their meaning. I am greatly surprised at this because I happen to know that many years ago the Americans displayed purely American daring and wanted to compose a psychological dictionary. But, as conditions were then, this was an impracticable task. For a long time no progress was made with the dictionary which passed through the hands of different editors. Finally, an energetic man was found; this was Warren, he is now dead, I think. Warren completed the editing of the dictionary, but it is not worth spending money on it. It is no good, being a complete failure.[180]

I shall read to you the author's judgement on our conditioned reflexes. You will see his lamentable juggling with words. One cannot but shrug his shoulders in amazement.

First of all, he coined a new term to designate our conditioned reflexes. I do not know whether he uses the term implication for the first time or whether it has been used by others as well. This is a Latin word. In his terminology our conditioned reflexes are not associations but implications.

Here is what he writes. I shall take up some of your time, gentlemen, because he devotes three pages to the point.

"Implication is a process which is indispensable to our needs of adaptation. Without it we would not be able to avail ourselves of our experience. Our life would resemble Sisyphean labours: no acquisition would help us properly to choose the mode of further behaviour. Indeed, what would actually occur if we did not tend to ascribe necessity to the combinations and connections which arise before us, if we were not inclined to regard as indispensable attributes the qualities of the object we meet for the first time? How would we react to it if we met it a second time?"—Do you get the point?—"For example, we find a fruit in the forest and taste it. It is sour and unpleasant. Our spirit does not limit itself to associating the acidity with the form and colour of the fruit to a degree that would evoke memory of the acid-

ity upon meeting the fruit again." Thus, you see, for some reason it does not limit itself. It would seem that this is precisely what happens, that we do recollect that the sour taste is connected with the appearance. But Claparède affirms that it "does not limit itself."

. . . What does this mean? How can this be brought into accord? We recall that the acidity in question is connected with the form and the colour; however, according to Claparède, the matter is not confined to this.

Further, he states: "If this implication were not itself implicated in the first relation experienced by us, then what basis would there be for a reaction in future?" What is this? Word-play? Instead of saying that these phenomena are interconnected, he affirms that if they were not implicated in this relation we should have no basis for further reaction! This is something incomprehensible.

A veritable deluge of words follows.

"Implication is based on the law of reproduction of the analogous which expresses the fact that the individual tries to repeat the previously favourable reactions of the past, to repeat them under identical or analogous situations. At the same time implication is a principle of generalization and induction which takes place in accordance with the law of reproduction of the analogous."

Reading this one would say: "Good gracious! What profound wisdom! It passes my comprehension." In reality, however, this is sheer nonsense, nothing but a cloudy haze. I beg your pardon, later on you will see this for yourselves. The ordinary man would think: "Evidently I lack education. That is why I know nothing about this and cannot understand it." But my conviction is that this is simply playing with words. . . .

"The reaction to a new situation on the basis of old experience—'experience meaning association'—clearly shows us that implication roots in the motor layers of being." What is one to make of all this? (*Laughter.*) He neither explains nor proves anything, all he does is to trot out a phrase!

But there is worse to come: "One can say that life implicates implications." Indeed, this is an intolerable word-play. What does it mean?

... "Implication is not a tardy, evolved and superior phenomenon ... this is clearly demonstrated by the conditioned reflexes." What do you think of that? Are not all conditioned reflexes gradually formed, developed and reinforced before our eyes?

"Usually they are regarded as a peremptory argument in favour of the doctrine of association." He is most anxious to uphold this association. And without a moment's hesitation our conditioned reflexes and associations are bundled into implication, with the result that we get not association but implication.

I have read the three pages and see no grounds for making any distinction between implication and association, especially since he is speaking about our facts.

"While implication is determined by the notion of adaptation, it is governed by the need of adaptation, it produces a certain action. To implicate means to await; and this, in its turn, means to strive for that which you await." Is not this sheer twaddle? Gentlemen, there are many of you here. Who can find in these three pages the slightest grounds for making any distinction between associations and these implications? So far as I am concerned I fail to see any, although I have read the passage more than once.

E. A. Asratyan: The main thing is that he has not understood the conditioned reflexes.

I. P. Pavlov: No, that is over-simplification. I cannot agree with you.

... Undoubtedly, this is a special breed of people, a special sphere in which there is no place for genuine thought, where it is always buried in the devil knows what. That is quite clear.

... No, here it is not a matter of lack of knowledge. It is a matter of playing with words. These gentlemen never bother about the real meaning of their words, they are

unable to give words a concrete sense. That is the main point. They really have a specific tendency to play with words, while ignoring reality. Our controversy with Claparède has been going on for twenty years. You probably remember that Zeliony translated his first ideas and that right from the very beginning I resolutely opposed zoo-psychology. Now the position is this: we accumulate a multitude of facts and systematize them, disregarding psychology completely. All this takes place before his eyes, and he constantly studies it. No, lack of knowledge is out of the question, since our controversy has been in progress for more than twenty years.

It follows that psychological thought is quite a peculiar matter; it does not regard words as signs and does not observe the principle that in using words one must always remember the reality implied by them. But Claparède does not adhere to this rule and has no desire to do so. There can be no other interpretation.

[CONCERNING KRETSCHMER'S BOOK *PHYSIQUE AND CHARACTER*[181]]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON OCTOBER 23, 1935

Academician *I. P. Pavlov*: . . . Not long ago I took another look at Kretschmer's book *Physique and Character*. I read it when it first came out; at that time I said more than once that it puzzled me. Kretschmer committed an error (despite the fact that he is a talented man, and possibly even because of his artistic talent) by trying to reduce all humankind, all the inhabitants of the globe, to two of his clinical types: schizophrenics and cyclothymics. Of course, this is a strange approach to the problem: why should the types which predominate in certain disorders and which sooner or later find their way to the psychiatric hospital be regarded as the fundamental human types?

Actually the majority of mankind has no relation whatever to these hospitals. That is his obvious error; carried away by the clinic he overlooked the rest of the world.

I failed to understand why all outstanding personalities must necessarily be regarded either as schizophrenics or as cyclothymics. I put the same question to others but nobody could help me to attain comprehension; so I gave up the attempt as hopeless.

Now, ten years later, when typology has made considerable progress, I decided to read the book once more, but the task proved impossible, I had to abandon the intention. It is an absolutely fruitless occupation. His book cannot be comprehended because it is permeated through and through with his fundamental error: he wants to reduce everything to his two types. However, even the dogs showed us that there exist not two, but at least four types. Besides, he never deals with normal individuals, does not think or speak of them.

There is another strange thing about his book. He makes no distinction between type and character, and that, too, is a blunder.

Nowadays we firmly adhere to the view that man has inborn qualities and, on the other hand, qualities that he has acquired in the course of life. That is clear. Consequently, if we are dealing with inborn qualities, this would be a matter of the type of nervous system, and if we are dealing with character it would be a matter of a combination of inborn inclinations and those acquired during lifetime under the influence of diverse impressions.

Therein lies his error: he has confused everything; he makes no distinction between the study of the inborn type and the qualities acquired by man in the course of life.

Let us turn now to our dogs. We always relate the study of types to three phenomena: to the strength of the opposed nervous processes, to their reciprocal equilibrium (equilibrated and unequilibrated types) and, finally, to their mobility.

On the other hand, we also possess data indicating the factors which constitute character.

Take, for example, the dog "Ratnitsa." "Ratnitsa" belongs to the strong type, but, as the experiments revealed, her character prevents her from working in an ordinary chamber, since everything distracts her to no purpose.

We can point to another phenomenon of great importance for the dog's character and which imparts to it a strictly definite physiognomy.

We encountered this phenomenon for the first time some years ago. We had two dogs which exhibited a strongly pronounced guarding reflex. They recognized only one person—their mistress—with whom they were most friendly, and who could do anything she liked with them. To all others they were ferociously hostile. But this connection with the mistress manifested itself only under certain conditions.

Now let us take the dog "Ussach," whose behaviour has been thoroughly studied by us. When placed in the stand in an isolated chamber with M. K.[182] in front of him, no one else could approach him. It was an ordeal to sit beside M. K. and attend the experiment. The dog would bark furiously and gave the impression that he would tear me to pieces were he to break loose.

But the moment it was led out of the room, its attitude to people abruptly changed. What convincing proof of adaptation to definite conditions!

At the moment V. K.[183] has a similar kind of dog in his laboratory, and none but he can treat it, since it is ready to bite anyone who tries to approach.

Consequently, this is a particular dog and its behaviour underlies a special trait in its character—its ferocity.

An interesting point is that there is a special condition which reconciles the dog to V. K.—the noose of a rope thrown around its neck with the end held by V. K. At first nobody could approach this animal. Then the noose was thrown round its neck through the grille of the cage, with

V. K. holding the end of the rope. It was this act that gave him power over the dog. From that time on he was able to lead the dog, to make it obey orders, etc. Thus you see to what a great degree this is specialized.

In this connection I recall a past impression. In the courtyard of our house in Ryazan we kept a dog in a kennel. Since we wanted it to be a good watch-dog not everybody was allowed to approach it. The janitor alone was accorded the privilege of chaining and unchaining it. It was ready to tear anybody else who would try to approach it. But a dog of this kind rushes at everybody only when it is chained; the moment the chain is taken off, it pays no attention to anyone, it simply enjoys its freedom.

Thus we have here, on the one hand, a pronounced trait of the character, and, on the other hand, an acquired quality.

The guarding reflex is an excellent illustration of a trait of character, but not of type. Similarly, the passive-defensive reflex is not a trait of type, but of character, and is acquired in the course of life.

[THE INFLUENCE OF THE IDEALISTIC WORLD OUTLOOK ON THE ATTITUDE OF SCIENTISTS TOWARDS THE THEORY OF CONDITIONED REFLEXES]

EXTRACT FROM THE STENOGRAPHIC RECORD OF THE "WEDNESDAY," HELD ON NOVEMBER 6, 1935

Academician *I. P. Pavlov*: As you are aware—I mentioned this in dealing with the history of the theory of conditioned reflexes—our conditioned reflexes encounter strong opposition in the heads of those imbued with dualism. What is taking place here is a collision between physiological law and psychological law, between the dualistic conception and the monistic conception of man. I am speaking of the fact which I pointed out a long time ago and which I recently included in my lectures at the Institute for Perfection of Physicians. The attitude of some people to our physiology

of the higher nervous activity—and who will deny that this is physiology?—is quite different. You probably remember that in my first laboratory for the study of conditioned reflexes one of my colleagues resented our attempts, our new methods of studying the behaviour of dogs. He is still going strong and feels somewhat ashamed when we meet.

On the other hand, the Englishman Sherrington displays similar scepticism. In 1912, in the course of conversation he said to me: "You know, your conditioned reflexes would hardly be popular in Britain, because of their materialistic flavour," because they oppose the dualistic concept. There you have the reason for his unbelief; this is confirmed by the lectures[184] which he delivered last year and in which he manifested his dualistic concept by affirming that man is a complex of two substances: the supreme spirit and the sinful body. Strange as it may seem for a modern physiologist, he clearly says that there is probably no connection between the mind and the brain. . . .

. . . We must understand that the conditioned reflexes occupy an exceptional place in the world of physiology because there is a dislike for them on the part of many who have a dualistic world outlook. This is quite obvious. The conditioned reflexes force their way to the forefront. They wage a continuous fight against this dualism which, of course, does not surrender.

This is seen in greater or lesser degree from the fact that the conditioned reflexes are accepted by physiologists with a certain reluctance. Strange as it may seem, many physiologists, authors of text-books, do not cite any data concerning our experiments with conditioned reflexes. Not long ago Heber's reputable manual was translated in Moscow; this manual makes no mention of the conditioned reflexes. In view of this, Prof. Shaternikov, the editor, specially commissioned one of us to write a chapter on the subject. The same thing can be observed in other text-books where practically no mention is made of conditioned reflexes. This

shows how deeply dualism is rooted in the minds of scientists.

In the category of such scientists one can include, for instance, Mr. Bethe, a rather prominent German physiologist from Frankfort on the Main. I think that his battle against conditioned reflexes has led him to commit a serious error in his work, although, generally speaking, he is an able man. E. A.[185] is now correcting him; this, naturally, will put him to shame and make him remember that his general world outlook should not be brought into scientific thought. For the time being these are quite different things.

Bethe destroyed the extremities of dogs partially or fully and in various combinations.

Naturally, after each mutilation the dogs were disabled for a time, depending on the operation. Subsequently, however, the disability was gradually obliterated, and the dogs recovered their ability to move, sometimes even in a quite satisfactory way; in other words, their locomotion was restored.

But this can be observed in human beings. And, as I have already said, these experiments were undertaken to no purpose whatever. When Bethe described them in Stockholm in 1926, I, sinner that I am, was indignant: what on earth makes you mutilate the unfortunate dogs? It is of no value at all and it does not prove anything.

The entire mass of human beings know this from their own experience. Was it worth while then to cripple twenty or thirty dogs merely for the sake of reproducing this fact?

Bethe's analysis of this phenomenon as applied to dogs is very simple. He ascribes everything to the plasticity of the spinal cord. It is a well-known fact that any mutilation becomes levelled out with the passage of time. But to this he added the words: "This must be ascribed to some mysterious property (since it is without further analysis) of the spinal cord." This is what all his works and all his talk about plasticity is reduced to. I am likewise inclined to see in this a manifestation of dualism. After all, what did he

achieve by his absolutely useless experiments? Nothing, and yet he found some followers. There is no gain here at all, only idle talk. The dualistic ardour directed against monism, which manifests itself in our conditioned reflexes, has clouded Bethe's mind to the degree that the idea of the necessity of devoting attention to the conditioned reflexes did not even enter his mind. But, as a matter of fact, everything he said about the plasticity of the spinal cord is fully applicable to our cortical conditioned reflexes as well. Consequently, the first thing that needed doing, had he not been under the spell of the dualistic world outlook, and had he paid attention to our conditioned reflexes, was to put the question, after the dogs had learned to move again and recovered their faculty of locomotion: will they preserve this faculty if their cerebral hemispheres are removed? Had he done so everything would have been reduced to the conditioned reflexes of the cortex. But he did not do that. E. A. did so, and he proved to be absolutely right. All the dogs recover their locomotion with the help of the cerebral hemispheres, i.e., with the help of the conditioned reflexes. If a mutilated dog which has subsequently learned to move anew, is deprived of the cerebral hemispheres, it will become an incurable invalid.

So you see how Bethe plays on words, being fully satisfied with them, how he uses the term "plasticity" and rests content with it.

That is a very instructive fact. . . .

NOTES AND COMMENTARY

I

PUBLIC AND SCIENTIFIC SPEECHES AND ADDRESSES

Being a successor to the progressive traditions of the revolutionary Russian intelligentsia of the 19th century, Ivan Petrovich Pavlov throughout his entire lifetime was a partisan of true democracy; he frequently opposed the reactionary tsarist officials, and fought vigorously for a progressive science free from prejudice.

In the period of Soviet rule Pavlov became an ardent supporter of the new regime created by the Great October Socialist Revolution under the wise leadership of the Communist Party. The great scientist became an outstanding public figure. In his brief, but brilliant addresses, speeches and letters he revealed his attitude towards social developments, his firm belief in the great historical role of his motherland, in the greatness of the Russian people.

[1] The salutatory message of Pavlov, then President of the Organizing Committee of the First Sechenov Physiological Congress, was published in the Russian Physiological Journal, Vol. I, Nos. 1-2, 1917. Pavlov, jointly with V. I. Vartanov and A. A. Likhachev, spent years in vain attempts to obtain permission from the authorities to convene an all-Russian congress of physiologists. This proved possible only after the overthrow of tsarism. Pavlov was unable to attend the congress because of a fractured thigh, the result of a fall on December 27, 1916. p. 49

[2] The apocryphal phrase "The republic needs no science!" ("La République n'a pas besoin de science!"), usually attributed to the president of the revolutionary tribunal which tried Lavoisier, is, apparently, simply an invention of bourgeois historians; the latter, pursuing their own ends, frequently used this phrase in a tendentious way, to discredit by all means the era when the poor classes of town and countryside in France appeared on the historical arena. K. A. Timiryazev, outstanding Russian naturalist, correctly stated: "The French people,

driven to despair by foreign intervention, treason within and ruin, but ready for new sacrifices, prosecuted in the person of Lavoisier a representative of the hateful estate of tax-farmers, whom they regarded as internal enemies and accomplices of their foreign enemies. Lavoisier was one of the twenty-six fermiers généraux, or tax-farmers, who mounted the guillotine on that day; he paid for the misdeeds of others, for the misdeeds of generations of plunderers who had been sucking the blood of the French people. There is no doubt that he was not guilty of their crimes. . . ." (K. A. Timiryazev, *Complete Works*, Vol. I, 1937, p. 215.) p. 50

[3] The letter was written by Pavlov in reply to congratulations from the Academy of Sciences of the U.S.S.R. on the occasion of his 85th birthday. p. 51

[4] The letter is a reply to a telegram sent by the members of the Leningrad Sechenov Physiological Society on the occasion of a special session dedicated to the 85th birthday of Academician I. P. Pavlov, founder of the society and its honorary president. p. 52

[5] Reply to greetings sent by the Presidium of the Academy of Sciences of the U.S.S.R. on the occasion of I. P. Pavlov's election to the post of Director of the Institute of Physiology and Pathology of the Higher Nervous Activity. p. 53

[6] The letter to the youth was written by Pavlov in connection with the request of the Central Committee of the Leninist Young Communist League of the Soviet Union that he should say something about the tasks of young scientists. p. 54

[7] The Fifteenth International Physiological Congress was held in 1935 in Leningrad and Moscow. Pavlov was president of the Organizing Committee of the congress and its honorary chairman. p. 56

[8] In August 1935 Academician Pavlov visited his native town—Ryazan. The District Executive Committee of the Soviet of Working People's Deputies gave a banquet in honour of the great Soviet scientist which was attended by representatives of the local community—physicians, teachers and others. This speech was Ivan Petrovich's response to the congratulations. p. 60

[9] July 1935. This is what Pavlov said about the prospects of his work. He had just recovered from a long and serious illness. p. 61

II

WORKS ON BLOOD CIRCULATION AND THE TROPHIC ACTION OF THE NERVOUS SYSTEM

I. P. Pavlov's works on the circulation of the blood relate to the earliest period of his activity (1874-1889); they are of great interest since they reveal true richness of observation, perfect technique of investigation, as well as audacity and originality in formulating the problems. In this experimental research the great physiologist, long before many foreign scientists, elucidated new and still unknown aspects of the reflex regulation of blood circulation. The fundamental idea of this research was the concept of auto-regulation of blood circulation in the integral organism aimed at maintaining the blood pressure at a definite level corresponding to the given conditions. This concept was confirmed by the discovery of centripetal nerves which reflexly accelerate the cardiac activity and thereby increase the blood pressure; their existence was first established in the work of I. P. Pavlov and V. N. Veliky. This discovery was an important addition to the so-called depressor nerve (i.e., the nerve declining the blood pressure) previously discovered by the Russian scientist Cyon and by K. Ludwig, a nerve the excitation of which provokes in a reflex way retardation of the cardiac activity and dilation of the vascular system.

It is worth noting that Pavlov's first works on blood circulation revealed his tendency to treat the organism as a single whole; they also reveal the exceptional importance which he attached to the nervous system in the regulation of the organism's functions.

Pavlov's ideas concerning the centripetal (sensory) nerves of the vascular system and of the internal organs, first formulated in these works, were further developed in the works of his disciple, Academician K. M. Bykov and his colleagues.

[10] This abstract of a paper prepared by V. N. Veliky and I. P. Pavlov was published in the *Collected Papers of the St. Petersburg Society of Naturalists, 1874*, Vol. V, p. 66. The problem of a reflex intensification of the cardiac activity and of an increase of the blood pressure was raised in this paper for the first time. In his student days V. N. Veliky (afterwards professor of the Tomsk University) worked with Pavlov in the laboratory of the prominent physiologist and histologist Academician F. V. Ovsiannikov. p. 65

[11] N. accessorius Willisi—the eleventh pair of craniocerebral nerves. p. 65

[12] Ganglion stellatum—the star-shaped ganglion, a big sympathetic ganglion from which the sympathetic nerves accelerating the cardiac activity originate. p. 65

[13] The inferior laryngeal nerve and the pneumogastric nerve. p. 65

[14] The article "Experimental Data Concerning the Accommodating Mechanism of the Blood Vessels" was published in *Pflüger's Archiv für die gesamte Physiologie*, Vol. 16, 1877, pp. 266-271. This work elucidates the idea of a reflex accommodation of the cardiac and vascular activity. The laboratory headed by Professor A. O. Ustimovich was one of the first Russian experimental laboratories in the sphere of physiology. Already this work reveals Pavlov's tendency, so characteristic of all his subsequent activity, to carry out investigations "on non-intoxicated intact dogs." p. 66

[15] Karl Ludwig—outstanding physiologist-experimentalist of the 19th century. The object of his research was blood circulation. Many Russian physiologists, among them I. M. Sechenov and I. P. Pavlov, worked in his laboratory. p. 66

[16] Curarization—a motor paralysis brought about by the introduction of curare into the organism. Curare, a poison used by Indians on their arrow tips, prevents the transmission of excitation from the nerves to the muscles. p. 67

[17] N. ischiadicus—the sciatic nerve; contains numerous afferent, i.e., sensory, fibres. p. 73

[18] The paper "Concerning Trophic Innervation" was read by Pavlov at a session dedicated to the 50th anniversary of scientific and medical activity of A. A. Nechayev, held on December 31, 1920 at the Obukhov hospital. It was published in the "Symposium of Scientific Works Dedicated to the 50th Anniversary of Scientific and Medical Activity of Prof. A. A. Nechayev," Part I, Petrograd, 1922.

The theory of the trophic action of the nerves, i.e., of their capacity to increase or decrease the vitality of tissues, is closely connected with the observations described by Pavlov as early as 1883 in his doctor's dissertation "The Centrifugal Nerves of the Heart." He discovered that along with nervous influences accelerating or retarding the cardiac activity there are nervous influences of the state of the heart's functional activity. Subsequently, the immense experience accumulated by Pavlov as a result of his observations on the effects of operations performed on the internal organs of dogs ("trophic reflexes," according to Pavlov), as well as his numerous observations on the changes which take place in the composition of the saliva under the influence of the secretory nerves, confirmed the existence of a trophic influence of the nervous system.

This theory greatly influenced the development of normal and pathological physiology by Soviet physiologists and clinicians. p. 74

III

WORKS ON DIGESTION

The present edition reproduces the first and the last chapters of I. P. Pavlov's classical work *Lectures on the Work of the Principal Digestive Glands*. These lectures were delivered first at the Institute of Experimental Medicine, and later repeated in a concise form at the Military Medical Academy. Their publication in 1897 brought Pavlov world-wide fame; he was awarded the Nobel Prize in 1904 for his works on digestion—the highest scientific award of that time. Pavlov was the first to apply strict asepsis in physiological experimentation and to elaborate delicate surgical methods, as well as highly complicated operations on different parts of the digestive canal; this enabled him to study the secretion of digestive juices in conditions of normal activity of the animal organism, taking into consideration the finest relations between the functions of the different organs and the influence of the external environment. In the course of ten years Pavlov actually created anew the modern physiology of digestion. At the same time these works marked the beginning of a new surgical trend in experimental biology. They have played and still play a prominent role in the solution of important practical problems in medicine and animal husbandry. Thanks to the application of Pavlov's methods, the Soviet physiologists have made a considerable contribution to the knowledge of the laws of digestion and livestock feeding.

The first, introductory lecture describes the new methods of integral study of physiological processes in conditions of a chronic experiment. The eighth and last lecture gives a profound theoretical interpretation of the material, shows the causal dependence of the secretory and motor activity of each section of the digestive canal, the biological expediency, and the adaptability of the glandular activity to the given alimentary conditions.

The lectures distinctly reveal Pavlov's constant tendency to link the results of physiological research with the practical tasks of the clinic.

[19] Brücke—a prominent German physiologist of the 19th century. p. 86

[20] Claude Bernard (1813-1878)—celebrated French physiologist, one of the founders of experimental physiology. The method of obtaining pancreatic juice was described by him in his book *Leçons de physiologie opératoire*, 1879. p. 87

[21] Rudolf Heidenhain—outstanding German physiologist who devoted much attention to the study of digestion. In 1884-1886, while on a scientific mission abroad, Pavlov worked in Heidenhain's laboratory. Heidenhain actually repeated the operation of a permanent pancreatic fistula which Pavlov had performed earlier. p. 88

[22] The American physician Beaumont studied for nine years the process of digestion in the organism of a Canadian hunter whose stomach had an unhealable fistula as a result of an accidental wound. His work was published in Boston in 1834. p. 93

[23] In 1889 Minkovsky (jointly with Mehring) extirpated the pancreas for the first time and proved the connection of diabetes with the activity of this gland. Minkovsky also studied the external pancreatic secretion. p. 102

[24] The operation of a fistula between the inferior vena cava and the vena portae, or of the so-called Eck fistula, was proposed by the Russian surgeon Eck in 1877. The method of the operation consists in the formation of a communication between the two veins in question and in a simultaneous ligature of the vena portae above the fistula. Owing to this, the blood current is transmitted from the alimentary canal direct into the inferior vena cava, without passing through the liver. Pavlov thoroughly studied the effect on the organism of the exclusion of the liver and made considerable improvements in the method of the fistula operation; consequently, this operation must be actually called the Eck-Pavlov fistula. (See the following works by I. P. Pavlov: "The Eck Fistula Between the Inferior Vena Cava and the Vena Portae, and Its Effect on the Organism" (jointly with M. Gan, V. Massen and M. Nentsky), *Complete Works*, Vol. V, 1949, pp. 3-25; "A Modification in the Operation of an Eck Fistula Between the Vena Portae and the Inferior Vena Cava," *Ibid.*, pp. 34-35; "Remarks on the Eck Fistula from the Surgical Point of View," *Ibid.*, pp. 36—38.) p. 102

[25] Pavlov's Nobel speech was first published in the symposium "Les prix de Nobel en 1904," Stockholm, 1905. This speech expounds the fundamental principles of the physiology of digestion from the integral biological point of view which became predominant in this branch of science thanks to the works of I. P. Pavlov and his school; in addition, the Nobel speech is of considerable interest since it establishes a direct succession between Pavlov's study of the digestive process and his passage in the beginning of the 20th century to the study of the higher nervous activity. p. 129

[26] In vitro—literally in glass, i.e., outside the organism. p. 130

[27] Pia desideria (Latin)—pious desires. p. 138

[28] Markel Vilhelmovich Nentsky (1847-1901)—celebrated biochemist. From 1891 to 1901 he was in charge of a division at the Institute of Experimental Medicine. Pavlov, jointly with M. Nentsky, carried out a number of investigations relating to the role of the liver in the formation of urea in the organism. The works of M. Nentsky on the chemical relationship between the blood pigment of animals—haemoglobin, and the vegetable pigment of plants—chlorophyll, were highly appreciated by K. A. Timiryazev. p. 140

IV

THE PROBLEM OF THE STUDY OF HIGHER NERVOUS ACTIVITY AND THE WAYS OF ITS EXPERIMENTAL SOLUTION

[29] This speech was delivered at a plenary session of the International Medical Congress in Madrid, in April 1903. It was first published in the "Proceedings of the Military Medical Academy" 1903, p. 103.

Having elaborated special methods for the study of reflex relations in the organism and of the secretion of the digestive glands in a practically normal animal, Pavlov already in his works on digestion pointed out the possibility of "psychical secretion" along with the secretion conditioned by purely physiological factors. At the end of the nineties, Pavlov began his experimental study of the mechanism of "psychical secretion."

The famous Madrid speech contains the maximum programme which the great creator of the theory of the higher nervous activity set himself and which he consistently and steadfastly carried out in the thirty-three years of his subsequent purposeful scientific activity. This speech fully revealed I. P. Pavlov's materialistic attitude towards psychical phenomena: he considered psychical activity from the biological, evolutionary point of view and rejected the "mechano-physical" and vitalist views.

It is in this article that Pavlov first gave his definition of the conditioned and unconditioned reflexes p. 151

[30] Teleology—an idealist theory proclaiming that all phenomena existing in the world are conditioned by the influence of expediently acting forces which direct them to predetermined ultimate goals. p. 154

[31] Animism—a theory recognizing the existence of souls in inanimate objects. Pavlov considered animism equivalent to idealism. p. 154

V

METHODS OF INVESTIGATION AND FUNDAMENTAL LAWS OF DEVELOPMENT

[32] The "Lectures on the Work of the Cerebral Hemispheres" were delivered by I. P. Pavlov at the Military Medical Academy in the spring of 1924 for physicians and biologists. They were published in 1926 and republished without any changes in 1927 and 1937.

In the preface to the third edition Pavlov characterized the "Lectures" as "... a fundamental exposition of our facts, systematized for the first time, it covers more than three-quarters of the entire period of our work in the field of the physiology and pathology of the higher nervous activity."

In his "Lectures" Pavlov substantiated the physiological mechanisms which determine the properties of the higher nervous activity and indicated the vast prospects of utilizing laboratory experience in the neurological and psychiatric clinics. Since the factual material contained in these lectures was subsequently considerably supplemented by further research, which was generalized by Pavlov in corresponding articles and papers, the present edition contains only the first two chapters, brilliantly illustrating the history of the problem and the Pavlovian method of studying the higher nervous activity. p. 171

[33] Munk and Ferrier studied the functions of different parts of the cerebral cortex; they showed that the cortical zones, which do not react to electrical stimulations, perform definite functions, confined to the given region and connected with the reception of external stimuli (the visual zone, the auditory zone, etc.). Munk was the first to establish the existence of cortical regions with more complex sensory functions; the derangement of their integrity leads to the so-called "psychical" blindness or deafness when the patient sees the objects but can neither recognize nor name them. See also the article "Summary of Results of the Experiments with Extirpation of Different Parts of the Cerebral Hemispheres by the Method of Conditioned Reflexes," published in this edition. p. 172

[34] William James (1842-1910)—American psychologist, founder of the so-called philosophy of pragmatism, an idealistic system close to empirio-criticism, but possessing typical traits of the ideology of American capitalism. p. 174

[35] Wilhelm Wundt (1832-1920)—celebrated German physiologist and psychologist. He advocated the erroneous point of view that in studying the psychical activity of animals we should proceed from our own mental activity. p. 174

[36] René Descartes (1596-1650)—celebrated French philosopher and naturalist, a mechanist in the sphere of natural science and an idealist in philosophy. He was the first to establish the notion of reflex as an automatic reaction of the organism to an external stimulation, a reaction which takes place due to the transmission of excitation by the nerves to the brain. The mechanist nature of this notion was combined by Descartes with the concept of a human "intelligent soul."

See also the article "Reply of a Physiologist to Psychologists" published in this edition. p. 174

[37] Charles Sherrington—English physiologist known for his research concerning the reflex function of the spinal cord. His philosophical outlook is thoroughly idealistic. The reactionary character of Sherrington's views stands out with particular clarity in his latest works where he openly opposes Pavlov's theory of conditioned reflexes and affirms that psychical activity cannot be made known by the methods of natural science. Pavlov's attitude to Sherrington was made quite clear at the "Wednesday" gatherings (see pp. 563-69). p. 175

[38] R. Magnus—well-known Dutch physiologist who showed that the displacement of an animal in space (its "locomotor activity") and the distribution of tension (tonus) in the skeletal muscles are connected with reflex reactions, whose centres are located in the brain stem and the cerebellum. p. 175

[39] Comparative physiology is a branch of physiology which studies the functions of animal organisms at various levels of evolutionary development with the aim of disclosing the peculiarities of the unity of the organisms and of the environment at different stages of evolution, of determining the fundamental factors of development and reproducing the picture of evolution of physiological functions. Comparative physiology, which is based on a genuinely historical approach to the development of physiological functions in the organic world, has made particularly great progress in Soviet biological science. p. 176

[40] The theory of animal tropism was elaborated by the American physiologist-mechanist J. Loeb. According to Loeb, an organism exists in a medium with various lines of force (luminous rays, diffusive currents in the case of chemotaxis, etc.). The symmetrical structure of their bodies obliges the animals to orient them relative to these lines of force; otherwise one of the sides would be subjected to greater influence and this would lead to an intensification of physicochemical changes on this side, and would provoke more intensified movements. This is why animals always move to the source of stimulation along the straight line.

Loeb did not limit himself to lower animals; he applied his concepts also to higher animals emphasizing the forced character of their movements. He tried to explain the effect of a stimulus by its action on the muscular system of one of the symmetrical sides through the sense organs. Similarly he sought to explain by means of tropism even more complicated processes, such as instincts and conditioned reflexes, which he reduced to primitive physicochemical reactions. p. 176

[41] Herbert Jennings—American zoologist, known for his research into the physiology of reproduction and behaviour of lower animals. He was close to behaviourism, and his philosophical views approximated to pragmatism. Here, apparently, Pavlov has in mind his

work published in 1906 and entitled *Behaviour of the Lower Organisms.* p. 176

[42] E. Thorndike—American psychologist, one of the founders of the so-called "behaviourism," a trend in comparative psychology. (See note 44.)

Thorndike believed that apes and other animals solve any new problems arising before them by the method of numerous "trials and errors." Definite appropriate movements, accidentally effected by them, are fixed owing to associations which either persist or, on the contrary, disappear, depending on subsequent experience.

Pavlov had a high opinion of his research, which he regarded as the first attempt by a psychologist to make an objective study of the psychical activity of animals and as renunciation of the anthropomorphic views which attribute human motives to the behaviour of animals (see Pavlov's statements at the "Wednesday" gatherings). However, striving to give a single and universal explanation of habits, learning and intellect at all levels of evolution, Thorndike in a mechanist way equalized all animals and denied the existence of specific properties of human psychical activity. His book *The Process of Learning in Man* was translated into Russian and published in 1935. p. 176

[43] Concerning this episode Pavlov in his preface to *Twenty Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals* wrote: "I began to investigate the question of this (psychical—*Ed.*) stimulation of the salivary glands with my colleagues, Drs. S. G. Wolfson and A. T. Snarsky. While Wolfson collected new and important facts relating to the peculiarities of the psychical stimulation of the salivary glands, Snarsky undertook to analyse the internal mechanism of the stimulation from the subjective point of view, i.e., he assumed that the internal world of the dog (we performed our experiments on dogs)—its thoughts, feelings and desires—is analogous to ours. It was this that brought us face to face with an incident which had no precedent in our laboratory. We considerably diverged in our interpretation of this internal world; further attempts failed to bring us to a common conclusion, contrary to the usual laboratory practice, according to which new experiments undertaken by mutual consent generally led to a settlement of all differences and disputes. Dr. Snarsky clung to his subjective interpretation of the phenomena, while I, astonished at the fantastic character and scientific barrenness of this approach to the problem, began to seek for another way out from this difficult situation." p. 177

[44] Behaviourists—zoopsychologists who advocate behaviourism (a derivative from the word "behaviour"). The theory of behaviourism appeared as a reaction against the hitherto existing anthropomorphic

notions of the psychical activity of animals. The behaviourists strove to study the behaviour of man and animals by objective methods and to exclude from their explanation of the behaviour all psychological notions connected with consciousness (sensation, attention, will, etc.). From the point of view of the behaviourists, consciousness is behaviour and nothing else. They considered that their task was to study the relations between the stimulus and the reaction to it. The behaviourists utilized Pavlov's theory of conditioned reflexes; however, they simplified it and rendered it primitive, since they did not take into account the laws of the higher nervous activity disclosed by Pavlov and connected with the peculiarities of the physiological processes in the central nervous system. The behaviourists were equally incapable of appreciating the Pavlovian concept of the second signalling system, which qualitatively distinguishes the human psychical activity from that of animals.

Being far removed from the dialectical interpretation of the phenomena of life, the behaviourists take the mechanist view of the vital activity of animals; they reduce consciousness to latent motor reactions and biologize the human personality. p. 178

[45] The depressor nerve terminates in the walls of the initial part of the aorta. Its excitation, as proved by the investigations of Cyon, the Russian physiologist, provokes a reflex dilation of the vessels and a decline of the blood pressure, i.e., a depressor effect. p. 181

[46] V. I. Vartanov (1853-1919)—outstanding Russian physiologist, Professor at the Petrograd Women's Medical Institute. p. 194

[47] Bahnung—the formation of a path in the nervous system, the facilitation of conduction of a certain reflex reaction as a result of its frequent repetition. p. 197

[48] Pavlov's famous speech "Natural Science and the Brain" was delivered at the plenary session of the Twelfth Congress of Naturalists and Physicians in Moscow on December 28, 1909. It was first published in the *Journal of the Congress of Naturalists and Physicians* in 1909.

In this speech Pavlov substantiated the necessity of an objective approach to the study of psychics and brilliantly characterized the significance of conditioned reflexes as a biological act which creates the necessary conditions for a regular metabolism between the organism and the external environment. In this speech, along with the elucidation of the mechanism of a temporary connection, he also formulated the fundamental law of concentration and irradiation of the process of excitation in the cerebral cortex.

K. A. Timiryazev highly appraised Pavlov's speech. In this connection the following exchange of friendly letters took place between the two great Russian naturalists.

I. P. Pavlov wrote to K. A. Timiryazev:

"Dear Kliment Arkadievich,

"I left the Moscow Congress on December 29 and only yesterday I learnt from the *Journal of the Congress* of your appreciation of my speech. I deem it natural and appropriate to tell you of the great joy which your appreciation brings to my heart. Harmony of scientific thought and recognition by our colleagues of the correctness and value of our views are to us the most legitimate source of assurance and satisfaction. I am very conscious of these feelings, since, to my regret, I belong to the type who always tend towards uneasiness and doubt, which is, evidently, the result of my neurasthenia. Allow me to express my deepest gratitude.

"With all my heart I wish you full recovery and a rapid return to your usual activity.

"Respectfully and sincerely yours,

"Ivan Pavlov."

K. A. Timiryazev replied:

"Dear Ivan Petrovich,

"Words fail to express the joy and satisfaction which your kind letter brought me. When, under the deep impression made by your speech, I sent you my telegram, it occurred to me that the opinion of one who does not understand a thing about your problems would be of no importance to you. But later I consoled myself with the thought that no one can be prevented from expressing his admiration. Your friendly, comradely attitude has definitely reassured me and filled me with joy not only for myself but for our science in general. I, too, am obliged to wage a struggle against the botanists, young and old, Russian and German, who advocate the view that plant physiologists must renounce 'the strict rules of naturalist thought' and replace them by an absurd 'phytopsychology' which, fortunately, does not exist. Now that I can refer to the fact that you, the man recognized by the whole world as the 'great physiologist of the Russian land,' have undertaken to withdraw the psychological method from its last stronghold in physiology, I feel sure ground under my feet and new strength for further struggle.

"I regard your speech as a great event in the history of natural sciences and deeply regret that I could not attend the congress. In general, it was the thought that I would be able to see you and have a talk with you that attracted me most to the congress.

"Allow me to thank you once more from the bottom of my heart for your kind and warm letter.

"Respectfully and sincerely yours,

"K. Timiryazev."

p. 206

[49] The notion of "unconscious conclusions" was introduced by Helmholtz to designate the reactions elaborated as a result of repeatedly evoking a definite situation forgotten by a man. Owing to this the reactions penetrate unconsciously, against one's will. Helmholtz considered that the process of "unconscious conclusions," being the most elementary in the nervous activity, underlies the process of thinking. (See his book *Physiologische Optik*, second edition, p. 601.)

Being a dualist and a Kantian, Helmholtz was far from adhering to the materialist conception of psychical activity and this concept resulted exclusively from empirical observation, which was not developed in his works. Helmholtz's idealist theory of symbols was subjected to criticism by V. I. Lenin in his book *Materialism and Empirio-Criticism.* p. 215

[50] The article "'Pure Physiology' of the Brain" was a paper originally prepared by Pavlov for the Congress of Psychiatrists, Neurologists and Psychologists scheduled to be held in Switzerland in August 1914, but postponed on account of the outbreak of the war. The article was first published in the Russian magazine *Nature*, No. 1, 1917, pp. 27-38. p. 220

[51] Edouard Claparède, Professor of Psychology of Geneva University, was the president of the Organizing Committee of the Congress of Neurologists and Psychologists which was to be held in 1914. Pavlov refers to Claparède's article "La psychologie comparée est-elle légitime?" ("Is the Existence of Comparative Psychology Justified?") published in the *Archive de Psychologie*, Vol. V, 1905, p. 13. See also Pavlov's statement at the "Wednesday" gathering of March 27, 1935 (p. 611). p. 220

[52] The article "Relation Between Excitation and Inhibition, Delimitation Between Excitation and Inhibition, Experimental Neuroses in Dogs" was published in the *Skandinavische Archiv für Physiologie*, Vol. 47, 1926, pp. 1-14, and was dedicated to the memory of Robert Tiegerstedt, a well-known physiologist, professor of Helsingfors (Helsinki) University. The article appeared in Russian in the book *Twenty Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals*, 4th ed., 1928.

The article is of special interest, since it characterizes the evolution of Pavlov's views and gives a detailed analysis of one of the fundamental problems of the theory of conditioned reflexes, namely,

of the problem of the relations between the processes of excitation and inhibition in the cerebral cortex. It also stresses the importance of the law of irradiation of the inhibitory process in the cerebral cortex discovered by Pavlov.

According to Pavlov, it is precisely the relations between the excitatory and inhibitory processes, or their equilibrium, which determine all our behaviour, the normal and the pathological. p. 231

[53] This refers to the *Skandinavische Archiv für Physiologie*. p. 231

[54] Pavlov implies here the centres in the brain stem and in the cerebellum which regulate the displacement of the animal in space, as well as the equilibration and distribution of the muscular tension in the skeletal musculature. p. 243

[55] Pierre Janet—French psychologist and psycho-pathologist, Professor of Psychology at the Collège de France, Paris. He showed that the pathological phenomena which are observed in neuroses are of a psychogenic origin, i.e., are not accompanied by pathologico-anatomical changes. Janet was the first to establish the form of neurosis known as psychasthenia and connected with the weakening of the psychical tonus. In respect of his gnosiological views Janet is close to the philosophy of subjective idealism. p. 243

[56] Nikolai Evgenievich Wedensky (1852-1922)—brilliant Russian physiologist, professor of Petersburg University. Studying the development of excitation in the nerve fibres he demonstrated that excitation and inhibition are stages of a single excitatory process in the protoplasm of nervous formations both of a peripheral and central origin. Wedensky's classical work *Excitation, Inhibition and Narcosis* was published in 1901 and marked the beginning of a new progressive trend in the theory of the physiological nature of the inhibitory process as a stage of development of a single excitatory process.

Although Pavlov did not agree with certain points of Wedensky's concept, he highly valued his researches. In one of his works (*The Latest Successes of the Objective Study of the Highest Nervous Activity in Animals*) Pavlov wrote: "... while investigating these deviations in the direction of the preponderance of the inhibitory process and the weakening of the excitatory, we found that one of the discoveries of our distinguished, late physiologist N. E. Wedensky was absolutely justified. Wedensky did much to advance the physiology of the nervous system; he succeeded in bringing to light important facts, but for some reason he has not received due recognition in foreign scientific literature. He is the author of the book *Excitation, Inhibition and Narcosis* in which he described the changes in the nerve fibres caused by strong stimuli and distinguished several phases of these changes. It proves now that these peculiar phases are entirely reproduced also by the nerve cells if there is an intense collision between the

processes of excitation and inhibition. I have no doubt that after this the researches of Wedensky will receive the recognition they merit" (I. P. Pavlov, *Complete Works*, Vol. III, 1949, pp. 331-332). p. 244

[57] The article "The Conditioned Reflex," written by Pavlov in 1934 for the Big Medical Encyclopedia, gives an exceptionally profound and extensive review of the theory of conditioned reflexes, and shows the tremendous general biological importance of the principle of temporary connection, as well as the significance for psychology and psychopathology of the objective method of studying the higher nervous activity in animals. p. 245

[58] See note 33. p. 246

[59] The law of Weber and Fechner establishes a numerical interrelation between the intensity of stimulation and the strength of sensation. According to this law, the strength of sensation increases in proportion to the logarithm of the intensity of stimulation and not parallel to the variations of its absolute value. p. 246

[60] See note 35. p. 246

[61] See note 42. p. 247

[62] See note 47. p. 249

[63] See article "Open Letter to Pierre Janet" published in this edition. p. 265

[64] See note 83. p. 265

[65] See note 138. p. 266

[66] See note 87. p. 266

[67] The term circularity is usually employed to denote a peculiar constitution of the psychics which manifests itself in periodical fluctuations of mood. When these fluctuations overstep the normal limits, disorder sets in—the manic-depressive psychosis (see note 86). p. 269

[68] See note 84. p. 269

[69] See note 86. p. 269

[70] The article "Physiology of the Higher Nervous Activity" represents Pavlov's address to the Fourteenth International Congress of Physiology in Rome on September 2, 1932. p. 271

[71] The German physiologist F. Goltz, for the first time on record (1892). succeeded in keeping alive several dogs in which both cerebral hemispheres had been extirpated. The animals were able to move and to eat; they preserved their sense of smell, hearing, cutaneous sensibility and the muscular sense. They reacted to light but did not distinguish the objects. In 1912, at Pavlov's request, an operation for the removal of both cerebral hemispheres in dogs was carried out in his laboratory by G. P. Zeliony. The experiments proved that elabora-

tion of conditioned reflexes in such animals after complete recovery from the bilateral extirpation of the cerebral cortex, was impossible. p. 272

[72] The case of the "famous patient" was described by Dr. Strümpel of Leipzig. I. M. Sechenov cited this case as proving the fundamental idea of his brilliant work *Reflexes of the Brain*, according to which "all acts of conscious and unconscious life are reflexes by the nature of their origin."

In 1900 Sechenov wrote: "A case of this kind was recorded by physicians in Germany. It concerned a young man all of whose sense organs, with the exception of one eye and one ear, no longer functioned, the intact eye and ear being his sole means of communication with the external world. So long as the eye could see and the ear hear, he remained in an alert state. But whenever the physicians, in the course of experimentation, closed his intact eye and stopped his ear, he rapidly fell into a sleeping state from which he could be awakened by means of sensory influences upon these very organs." Sechenov cited a similar case related to him by S. P. Botkin. "The patient, a woman of education, retained only the sense of touch and the muscular sense of one of the arms. As testified by the personnel of the hospital, she was almost continuously in a state of sleep; she communicated with other people in the following way: a pillow was placed on her abdomen, then someone would take her hand which preserved sensibility and passing it over the pillow made her write the question to which she had to give an answer.... Is it possible, then, given these facts, to doubt that a wakeful state, with the interchange of all kinds of sensations which inevitably accompany it, is maintained by luminar, acoustic, thermic, olfactory and often mechanical, external influences upon the sense organs?... The loss of all senses must necessarily result in complete loss of consciousness, since consciousness is expressed by none other than sensations of which the individual is conscious. Complete loss of the senses is bound to be followed by deep dreamless sleep." "The Participation of the Nervous System in the Human Working Movements," 1900. I. M. Sechenov, *Selected Works*, Vol. I, U.S.S.R. Academy of Sciences, 1952. Moscow, pp. 511-512.

These views of Sechenov are in full accord with the propositions advanced by I. P. Pavlov. p. 278

[73] The notion of dynamic stereotype was examined in detail by Pavlov in his article "Dynamic Stereotypy of the Higher Part of the Brain" (see p. 448 of the present volume). p. 283

[74] Pavlov has in mind his article "Essay on the Physiological Concept of the Symptomatology of Hysteria" (see p. 516 of the present volume). p. 285

VI

THEORY OF ANALYSERS, LOCALIZATION OF FUNCTIONS AND MECHANISM OF VOLUNTARY MOVEMENTS

[75] The paper "Summary of Results of the Experiments with Extirpation of Different Parts of the Cerebral Hemispheres by the Method of Conditioned Reflexes" was read in 1911 at a gathering of the Society of Russian Physicians in Petersburg. It was first published in the *Proceedings of the Society of Russian Physicians in St. Petersburg*, 1912-1913.

The combination of the method of conditioned reflexes with the extirpation of certain parts of the cerebral hemispheres enabled Pavlov to study the problem of localization of functions in the cerebral cortex of the dog in quite a new way. An exposition of the principal results of this study is given in the present article. p. 289

[76] Gyrus sigmoideus, g.g. coronarius and ectosylvius—the sigmoid, coronal and ectosylvian convolutions situated in the anterior part of the cerebral cortex of the dog. Stimulation of these cortical zones by means of an electric current makes the animal's extremities or trunk move. p. 294

[77] The article "Physiological Mechanism of the So-Called Voluntary Movements" was published in the *Collected Papers of the Physiological Laboratories of Academician I. P. Pavlov*, Vol. VI, No. 1, 1936. p. 306

[78] Kinesthetic stimulations—signals entering the nervous system from the skeletal muscles. By means of these signals we can judge of the state of muscular contraction or relaxation, the position of the extremities, the resistance which they have to overcome, etc. The significance of the kinesthetic stimulations, which are also called "muscular sense," was first demonstrated by Ivan Mikhailovich Sechenov. Kinesthetic cortical cells are those to which impulses are transmitted along the paths conducting the muscular sense. p. 306

[79] Numerous tricks with the so-called "thought transmission" are based on this phenomenon p. 308

VII

THEORY OF TYPES

On the basis of the rich experience accumulated in the course of almost thirty years of work on conditioned reflexes, Pavlov and his school thoroughly elaborated the typology of the higher nervous activity in dogs. As pointed out by Pavlov, this typology coincided with the four human temperaments described by Hippocrates. Already in Pavlov's lifetime

investigation was started with the aim of elucidating the biological importance of these types in dogs, as well as the problem of their transformation and hereditary transmission. At present this work is carried on at the Pavlov Institute of Higher Nervous Activity in the village of Pavlovo (formerly Koltushi).

[80] The article "General Types of Animal and Human Higher Nervous Activity" was published in the form of a booklet in the series *Latest Papers on the Physiology and Pathology of the Higher Nervous Activity*, Paper No. III, 1935. p. 313

[81] Hippocrates' four temperaments are implied here. A more detailed description of them is given on p. 340. p. 315

[82] Stereotypy—uniform repetition of definite stimuli in one and the same succession and of corresponding reactions to them. p. 337

[83] Ernst Kretschmer—German psychiatrist, author of the well-known book *Physique and Character*, a dualist in the interpretation of the psychical activity. Criticizing Kretschmer's type Pavlov stresses only one weak point of this concept. It should be added that Kretschmer, like all bourgeois psychologists and psychiatrists, ignores the influence of social environment on man. He is an adherent of the anti-scientific Morganist anthropo-genetics. p. 340

[84] Cyclothymics, according to Kretschmer's classification, are individuals of a sociable, jovial disposition, energetic, sometimes violent; persons susceptible to manic-depressive psychosis usually belong to this type (see note 86). p. 340

[85] Schizothymics, according to Kretschmer's classification, are individuals of a reserved disposition, absorbed in their internal world, fantasts; persons susceptible to schizophrenia belong to this type. p. 340

[86] Manic-depressive, or circular, psychosis—a mental disorder characterized by interchange of periods of violent excitation and depression. p. 340

[87] Schizophrenia—mental disorder which manifests itself in hallucinations, fantastic emotions, and split personality, however, with the intellect remaining relatively unchanged. p. 341

VIII

PROBLEMS OF SLEEP AND HYPNOSIS

The works included in this chapter are a striking example of the useful role played by the Pavlovian theory in solving problems relating to the physiology of the nervous system. Basing himself on laboratory observations in the course of the elaboration of conditioned

reflexes in dogs, Pavlov developed an original theory of sleep, regarding it as inhibition originating in the cortex and spreading over the lower parts of the central nervous system. At the same time he showed that sleep and hypnosis are one and the same phenomenon, differing only in intensity and extensity of inhibition.

88 The Paper "Some Facts About the Physiology of Sleep" was read by Pavlov at a meeting of the Petrograd Biological Society in 1915. It was first published in French in *Comptes rendus de la Société de Biologie*, Vol. 79, 1916, pp. 1079-1084. The sleep of animals, which had long been an obstacle to the experiments with conditioned reflexes, itself became the object of investigation. It was found that both sleep and hypnosis could be evoked in dogs by means of conditioned reflexes. p. 345

89 The article "Concerning the So-Called Hypnotism in Animals" was published as a supplement to the proceedings of the Physics and Mathematics Department of the Russian Academy of Sciences, of November 9, 1921. p. 352

90 Experimentum mirabile—a "miraculous experiment," which was first performed in the 17th century by Athanas Kircher. A hen, suddenly placed on her back, remains for a long time in a state of stupor and immobility. p. 352

91 The article "Physiology of the Hypnotic State of the Dog" was first published in the *Collected Papers of the Physiological Laboratories of I. P. Pavlov*, Vol. IV, 1932. p. 354

92 Catalepsy—a state of stupor. p. 354

93 See note 149. p. 355

94 The subsequent works of Academician K. M. Bykov's school showed that the internal organs dispatch nervous impulses to the brain, signalizing their state; on the basis of these signals it is possible to elaborate conditioned reflexes, and the function of the given organ can be modified by means of conditioned reflex regulation. (See K. M. Bykov's *The Cerebral Cortex and the Internal Organs*, Moscow-Leningrad, 1947. State Medical Publishing House.) p. 363

95 The motor nerves stretching to the muscles originate in the nerve cells of the anterior horns of the spinal cord. p. 364

96 This is a reference to the article "A Brief Essay on the Higher Nervous Activity" written by Pavlov in 1930. In this article he explained why under hypnosis it is the strong stimuli which are subjected to inhibition first (as, for example, in the equalization and paradoxical phases). He stated "that the exhaustion of the cortical cell always results in the development of an inhibitory process in this cell. Thus the inhibition irradiating from the cells continually stimulated by the

conditions of the experiment is summated with the inhibition of the specially stimulated working cell proper and reaches here maximum intensity" (I. P. Pavlov, *Complete Works*, Vol. III, 1949, p. 403). p. 366

[97] A case analogous to war-time neurosis was described by V. V. Rickman in his article "Disclosure of Earlier Traces of Stimulation in the Centres of a Defensive Reaction as an Analogue of Traumatic Neurosis." (*Collected Papers of the I. P. Pavlov Physiological Laboratories*, Vol. IV, 1933, p. 102.) This work shows the prolonged and heightened excitability of the centres of defensive reaction after the action of a powerful destructive stimulus, as well as the conditions which make this state manifest. Hypnotic inhibition of the cortex is one of these conditions. p. 368

[98] The paper "The Problem of Sleep" was read by Pavlov at a conference of psychiatrists, neuropathologists and psychoneurologists in Leningrad in December 1935. The stenographic record of this paper was first published in the *Complete Works* of I. P. Pavlov, Vol. I, 1940. p. 369

[99] Narcolepsy—a periodically appearing overwhelming desire for sleep. p. 376

[100] Cataplexy—a state of stupor evoked in certain animals by extreme fright or arising under the so-called animal hypnotism, i.e., when the animal is forcibly kept for a certain period in an unnatural position. Some regard it as a state analogous to catalepsy which is peculiar to human hypnotic sleep. p. 376

[101] Fili olfactorii—olfactory fibres extending from the olfactory bulbs of the brain to the olfactory conches and mucous membrane of the nasal cavity. p. 376

[102] Corpora geniculata—geniculated bodies, formations in the brain stem which represent the intermediate centres of the auditory nerves (internal geniculated bodies) and of the optical nerves (external geniculated bodies). p. 379

[103] Encephalitic sleep—pathological sleep developing in a patient suffering from epidemic encephalitis. p. 380

[104] Hypothalamus—part of the diencephalon situated under the optic thalamus and forming the bottom of the third ventricle of the brain. In the hypothalamus are located the centres of many vegetative functions of the organism: water metabolism, thermo-regulation, etc. According to the data of Hess, obtained as a result of the stimulation of this region by means of an electric current, the "centre of sleep" is situated here, too. In epidemic encephalitis, which is accompanied by pathological somnolence, certain changes of the nerve cells are observed in this region. p. 380

[105] Rapport—special faculty of a hypnotized person for perceiving

in a selective way exclusively the words of the hypnotist without maintaining any contact with the rest of the external world. Pavlov showed that this state is not the exclusive property of hypnotic sleep, and that it is sometimes also observed in normal sleep. p. 388

IX

PHYSIOLOGY AND PSYCHOLOGY

Pavlov considered that the principal object of his study of the higher nervous activity in animals was to disclose the physiological laws of human psychical activity and to include psychology in the sphere of natural sciences. In this, however, he was far from being inclined to apply in a purely mechanical way to man the laws of higher nervous activity observed in dogs; he pointed out that the peculiarities of the human higher nervous activity "sharply distinguish man from other animals." He wrote: "It would be the height of presumption to regard these first steps of the physiology of the cerebral hemispheres—complete in relation to programme but not to content—as solving the grandiose problem of that supreme mechanism of human nature." (*Complete Works*, Vol. IV, 1949, p. 326.) Towards the end of his life, Pavlov, basing himself on a thorough study of human mental pathology and on a profound biological consideration of the problem of mental evolution, clearly formulated certain fundamental physiological distinctions between the higher nervous activity of man and that of animals. He wrote: "When the developing animal world reached the stage of man, an extremely important addition was made to the mechanisms of the nervous activity. In the animal, reality is signalized almost exclusively by stimulations and by the traces they leave in the cerebral hemispheres, which come directly to the special cells of the visual, auditory or other receptors of the organism. This is what we, too, possess as impressions, sensations and notions of the world around us, both the natural and the social—with the exception of the words heard or seen. This is the first system of signals of reality common to man and animals. But speech constitutes a second signalling system of reality which is peculiarly ours, being the signal of the first signals. On the one hand, numerous speech stimulations have removed us from reality, and we must always remember this in order not to distort our attitude to reality."

It should be pointed out that objective study of the second signalling system was but begun by Pavlov. The articles in this chapter indicate the ways of applying physiological methods to the study of the laws governing human mental activity. At the same time Pavlov vigorously combated the animism and dualism of psychologists who denied the material foundation of the psychical processes.

[106] The paper "Physiology and Psychology in the Study of the Higher Nervous Activity of Animals" was read at a meeting of the Petrograd Philosophical Society on November 24, 1916 It was published in the *Journal of Psychiatry*, No. 6, 1917, pp. 141-146. This paper combines scientific exactitude with brilliant popularization of the Pavlovian methods of objective study of the higher nervous activity designed for an audience not familiar with biology. The above journal also published the discussion which followed the reading of this paper and in which the celebrated neurologist V. M. Bekhterev, as well as the philosophers-idealists N. O. Lossky, A. I. Wedensky and others, took part. p. 391

[107] Modest Nikolaievich Bogdanov (1841-1888)—well-known Russian zoologist and traveller, Professor of Petersburg University. p. 391

[108] Retina—the part of the eye which is sensitive to light. p. 400

[109] The article "Reply of a Physiologist to Psychologists" was published in the magazine *Psychological Review*, Vol. 39, No 2, 1932, in connection with the works referred to in the text, namely, the article by Guthrie "Conditioning as a Principle of Learning" and Lashley's "Basic Neural Mechanisms in Behaviour." In this article Pavlov gave a particularly exhaustive formulation of the fundamental methodological principles underlying the reflex theory—the principle of determinism, the principle of analysis and synthesis, and the structural principle Pavlov counterposes these materialist principles to the confusing idealistic concepts of the idealists. In his reply published in 1934 in Volume 41 of the same magazine ("The Pavlovian Theory of Conditioned Reflexes"), Guthrie expressed his idealistic concepts in even more distinct form, insisting that it is impossible to disclose the nature of psychological processes by objective physiological methods. p. 409

[110] Pavlov repeatedly pointed to the indispensable existence of internal analysers (see his articles: "Summary of Results of the Experiments with Extirpation of Different Parts of the Cerebral Hemispheres by the Method of Conditioned Reflexes" and "The Physiology of the Hypnotic State of the Dog"). Whereas the external analysers link the organism with the external world, the internal analysers, which receive signals from all the organs and systems of the animal, enable the latter "to analyse also that which takes place inside it." p. 412

[111] That is, proceeds not from experimental facts, but from a preconceived point of view. p. 419

[112] Charles Spearman—psychologist, Professor of London University. Spearman's idealistic point of view, cited by Pavlov, char-

acterizes the vitalist conception of this bourgeois psychologist with regard to the nature of the intellect. p. 420

[113] Receptor apparatus—the sense organs or the sensory nerve endings. p. 423

[114] Afferent nerves—sensory or centripetal nerves conducting the excitation to the central nervous system. p. 423

[115] Efferent or centrifugal nerves conduct the impulses from the central nervous system to the effector organs (muscles, glands, etc.). p. 423

[116] The central nervous system (the brain and the spinal cord) consists of the white matter—the nervous fibres—and the grey matter in which the nerve cells are mainly concentrated. The grey matter forms the cerebral cortex, as well as the nuclei of the brain stem. p. 423

[117] Cyto-architectonics—branch of the histology of the nervous system which studies the cellular structure of the cerebral hemispheres. In animals and man there are different cortical zones which are distinguished by their structure and cellular composition. p. 423

[118] Ataxic patients—patients who suffer from tabes; owing to a deranged conduction of the muscular (kinesthetic) sense in the spinal cord, the normal co-ordination of movements is abolished in them. Such patients can effect well-co-ordinated movements only when they control them visually. p. 438

[119] Fovea centralis—the region of the retina most sensitive to light. p. 439

[120] Wolfgang Koehler, Professor of the Berlin Institute of Psychology. Basing himself on his own experiments, Koehler stressed the importance of integral structures in the behaviour of chimpanzees, and attributed to the latter human-like intellectual faculties. Koehler and his followers criticized associationism (i.e., the conditioned reflex theory of behaviour) and behaviourism. Koehler's observations formed the experimental base of the idealistic concept of contemporary bourgeois psychology, the so-called "Gestalt psychology." His book *Investigation of the Intellect of Anthropoids* was translated into Russian and published in 1930. Pavlov subjected Koehler's views to ruthless criticism (see his statements at the "Wednesday" discussions included in the present edition). p. 443

[121] The article "Dynamic Stereotypy of the Higher Part of the Brain" was a paper read by Pavlov at the Tenth International Congress of Psychologists in Copenhagen on August 24, 1932. It was published posthumously in the book *Latest Papers on the Physiology and Pathology of the Higher Nervous Activity*, Vol I, pp. 33-39.

In his paper Pavlov for the first time substantiated the notion of the so-called "dynamic stereotype" which, in his definition, is a well-co-ordinated and equilibrated system of internal processes; he also indicated the way to the study of the integral higher nervous activity in animals. p. 448

[122] The "literary inspirer," to whom Pavlov alludes here, is D. I. Pisarev, writer and sociologist. p. 452

[123] This is an extract from Pavlov's preface to the book by Prof. A. G. Ivanov-Smolensky *The Fundamental Problems of the Pathophysiology of the Higher Nervous Activity*, published by the State Medical Publishing House in 1933.

Pavlov's views on the "fusion" of the psychical and physiological, the subjective and the objective, reflected his consistent materialistic tendency to bridge the gulf created by the idealists between the objectively existing material reality and the human consciousness. Here again Pavlov expressed his fundamental idea of a material base underlying all psychical manifestations and of the possibility of making the higher nervous activity known by means of the method of conditioned reflexes which he created, reflexes which combine the features of subjective phenomena and those of an objective physiological process. Affirming the necessity of creating a materialistic psychology, based on the physiological laws of activity of the nervous system, Pavlov wrote: "I am convinced that sooner or later the physiologists studying the nervous system and the psychologists will become united in their close and common work.... The more attempts that are made in this direction, the greater the chances that we shall finally come together to our mutual delight and benefit." (*Complete Works*, Vol. III, p. 359.)

A profound analysis of Pavlov's views on the close connection between objective and subjective phenomena was given in the report delivered by A. G. Ivanov-Smolensky at the Joint Session of the U.S.S.R. Academy of Sciences and the U.S.S.R. Academy of Medical Sciences, devoted to the problems of the physiological teachings of Academician I. P. Pavlov (June 28-July 4, 1950). p. 454

[124] See also corresponding statement by Pavlov at one of the "Wednesday" gatherings, pp. 612-13, as well as note 180. p. 455

X

EXPERIMENTAL PATHOLOGY OF THE HIGHER NERVOUS ACTIVITY

The articles included in this chapter reflect Pavlov's tendency not to rest content with the experimental study of conditioned reflexes, but also to elucidate the causes of human nervous and mental disorders by means of the physiological laws of the higher nervous activity disclosed by him. The analysis of experimental neuroses gave rise to Pavlov's exceptionally fruitful notion of protective inhibition—a physiological mechanism protecting the weakened nerve cells from over-excitation and lesion. Protective inhibition underlies many pathological phenomena in mental disorders; at the same time, as demonstrated, artificial intensification of this process is a powerful therapeutic remedy in a number of nervous disorders.

Also of great value is Pavlov's analysis of the action of bromide and caffeine on the higher nervous activity, as factors which vary the relative intensity of the inhibitory and excitatory processes. These investigations make possible precise dosage of the above-mentioned preparations and their higher therapeutic effect. Thus, just as in the period of his research into digestion, Pavlov endeavoured to bring together physiology and clinical medicine. "We must be able to repair the damaged mechanism of the human organism on the basis of exact knowledge of it"—wrote Pavlov, and this was the motto of the great physiologist.

[125] A lecture delivered on May 10, 1934 at the Institute for Perfection of Physicians in Leningrad published in 1935 as a booklet. p. 459

[126] See Pavlov's statements on this subject at the "Wednesday" gatherings, published in the present volume. p. 460

[127] Extirpation—in this case removal of certain parts of the brain. p. 460

[128] Psychogenic diseases are those which result from psychical traumatism, not connected with pathologico-anatomic changes in the organs. p. 468

[129] Psychasthenia—literally "mental feebleness"—a functional nervous disorder pertaining to the group of so-called "psychoneuroses." It was first described by the French psychoneurologist Pierre Janet. The typical symptoms of this disorder are: feeling of inferiority, morbid diffidence, reasoning, obsessive ideas. Pavlov considered that the symptoms of psychasthenia depend on a pathological rupture between the first signalling system and the second, as well as between the latter and the subcortex. p. 469

[130] Hysteria—a functional nervous disorder pertaining to the same group of psychoneuroses as psychasthenia. Its symptoms are: high suggestibility and auto-suggestibility, leading to development of diverse disturbances of physiological functions. p. 469

[131] Pavlov was elected professor to the chair of pharmacology in the Military Medical Academy in 1890 and occupied this position up to 1895. p. 476

[132] Involuntary repetition of the same movements in certain diseases. p. 479

[133] Perseveration—forcible repetition of one and the same syllable, word or phrase as a result of certain disturbances of the cortical zones connected with speech. p. 479

[134] A paper on this subject was read by Pavlov on July 30, 1935 at the plenary session of the Second International Congress of Neurologists in London; it was published in the book *Twenty Years of Objective Study of the Higher Nervous Activity (Behaviour) of Animals*, 6th ed., 1938. p. 481

[135] Convulsive contraction of certain groupings of muscles which in hysterical persons sometimes lasts for months and years. p. 484

[136] Phobia—pathological imaginary fear. p. 484

[137] Catalepsy—the petrifaction of the entire body or of its parts in positions artificially imparted to them and a simultaneous loss of ability to effect voluntary movements. It is observed in the state of hypnosis, as well as in certain mental disorders (for example, catatonia). p. 484

[138] Catatonia—a mental disorder pertaining to the group of schizophrenia and accompanied by stupor, psychical depression and negativism p. 484

[139] A state of pathological excitation peculiar to the manic-depressive (circular) psychosis. p. 485

[140] The article "Fusion of Principal Branches of Medicine in Modern Experimentation as Demonstrated by the Example of Digestion" is a paper read by Pavlov at a special meeting of the Society of Russian Physicians dedicated to the memory of S. P. Botkin in 1899. It was first published in the *Proceedings of the Society of Russian Physicians*, 1900, Vol. 67, November-December, pp. 197-242. p. 487

XI

PHYSIOLOGY AND PSYCHIATRY

Pavlov regarded a disease as a state of the organism in which specific relations arise between different organs and systems, and which cannot always be reproduced in experimental conditions. Accord-

ing to Pavlov, "...clinical practice will always be an abundant source of new facts. It is, therefore, quite natural that the physiologist should desire a closer union between physiology and medicine." This desire is expressed by Pavlov with particular force in his paper "Psychiatry as an Auxiliary to the Physiology of the Cerebral Hemispheres," read in 1919.

Taking into consideration the specific property of the human higher nervous activity, which distinguishes man from higher animals, Pavlov by no means regarded the data obtained in the laboratory (experimental neuroses in dogs) as fully explaining the disturbances in human mental activity.

He emphasized the existence "of specifically human neuroses"—psychasthenia and hysteria. The latter circumstance gave him an added interest in psychiatry, which, according to him, is an auxiliary in the study of the physiology of the cerebral hemispheres and helps to comprehend certain aspects of the higher nervous activity peculiar to man.

The objective approach of the physiologist-materialist in studying symptoms of mental disorders enabled Pavlov to elucidate a series of pathological processes in man, proceeding from the fundamental laws of the higher nervous activity previously disclosed in the course of experimentation; it also enabled him to indicate new and effective methods of treatment, now being successfully elaborated by Soviet clinicians.

141 The article "Psychiatry as an Auxiliary to the Physiology of the Cerebral Hemispheres"—originally a paper read by Pavlov at a meeting of the Society of Psychiatrists in Petrograd in 1919. It was published in the *Russian Physiological Journal*, Vol. II, 1919, pp. 257-260. p. 499

142 Tonic reflexes—reflex augmentation of tension in certain groups of the skeletal muscles—one of the symptoms of catatonia. p. 501

143 Maurice Schiff (1823-1896)—Swiss physiologist, who made a study of the central nervous system and the trophic action of the nerves on the tissues. p. 501

144 Decerebration—removal of the cerebral hemispheres and of the anterior parts of the brain stem in animals by means of sectioning the brain stem at the level of the anterior edge of the pons varolii. In this connection the tonic reflexes, whose centres are situated below the level of the sectioning, become intensified. p. 501

145 Progressive paralysis—an affection of the nervous system accompanied by profound anatomical changes in the cerebral cortex and developing in certain cases of syphilis. p. 505

[146] Thrombosis—obstruction of a blood vessel by a clot of blood, or by the so-called thrombus. p. 505

[147] The article "An Attempt of a Physiologist to Digress into the Domain of Psychiatry" was published in the booklet *The Physiology and Pathology of the Nervous Activity*, Moscow-Leningrad, 1930. Brilliantly proving his idea of the unity of physiology and pathology, Pavlov interprets the catatonic stage of schizophrenia as "chronic hypnotic inhibition" protecting the cortical cells of the patient's weak nervous system, which are open to injury, from further destruction. p. 509

[148] Hebephrenia—a form of schizophrenia characterized by impoverishment of mental life and nonsensical silly mannerisms. p. 509

[149] Negativism or contralism—a negative attitude towards the influences of the surrounding world; one of the fundamental symptoms of catatonia and of other schizophrenic forms; it is also met with in other mental disorders. p. 511

[150] Echolalia—automatic repetition by the patient of words heard by him. p. 512

[151] Echopraxia—automatic repetition by the patient of the actions of other people. p. 512

[152] The article "Essay on the Physiological Concept of the Symptomatology of Hysteria" was published in booklet form by the U.S.S.R. Academy of Sciences (1932, 36 pages). Professor A. V. Martynov, to whom this work is dedicated, had operated on Pavlov for gallstones. p. 516

[153] Concerning the centre of sleep, see "The Problem of Sleep" in this volume. p. 521

[154] Pierre Janet regarded hysteria as a derangement of consciousness, mainly as its splitting, which leads to the emergence of symptoms characteristic of this state. Janet attributed great importance to weakness of the nervous system and to emotions in the development of hysteria. p. 526

[155] The German psychiatrist Prof. A. E. Hoche in an article "Ist die Hysterie wirklich entlarvt?", published in the *Deutsche Medizinische Wochenschrift*, 58, p. 1, 1932, endeavoured to prove that no progress had been made in the comprehension of hysteria (see p. 540). p. 532

[156] Anaesthesia is total loss of cutaneous sensibility (the opposite state is hyperesthesia—heightened sensibility). Analgesia is incapacity to feel pain. p. 533

[157] Babinsky believed that suggestion and auto-suggestion play the principal role in the development of hysteria. p. 534

158 Eudetism is a specific psychical phenomenon which is close to the memory of images, i.e., when the image of an object persists long after the disappearance of the object from the field of vision. Eudetism is a normal phase in the development of memory, through which all children pass at a certain age. p. 535

159 Puerilism—a form of hysteria characterized by a naive, puerile conduct. p. 538

160 Paresis—incapacity to effect voluntary movements. p. 538

161 The article "Feelings of Possession (Les sentiments d'emprise) and the Ultra-Paradoxical Phase" was published in the *Journal de Psychologie*, Nos. 9-10, 1933, pp. 849-854. Pierre Janet was one of the editors of this journal. p. 542

162 Ambivalency—a symptom of schizophrenia when the patient simultaneously experiences diametrically opposed emotions (for example, joy and sorrow, etc.). p. 546

XII

FRAGMENTS OF STATEMENTS AT THE "WEDNESDAY" GATHERINGS

STRUGGLE OF I. P. PAVLOV AGAINST IDEALISTS

Pavlov's famous "Wednesdays" began in the spring of 1921 when, after the victorious termination of the civil war, the Soviet country embarked on the path of peaceful and creative work aimed at building the new, socialist society. Of particular significance for the rapid restoration of the normal activity of Pavlov's laboratories was the special decree issued by Vladimir Ilyich Lenin on January 24, 1921, providing for favourable conditions for the scientific work of Pavlov and his colleagues.

Twice weekly (on Wednesdays and Fridays from 10 o'clock in the morning till noon), with the strict punctuality for which he was renowned, Pavlov visited the small physiological laboratory of the Russian Academy of Sciences, which he headed and which occupied several rooms in the main building of the Academy overlooking Mendeleyev Avenue.

Taking part in the usual laboratory experiments of his small group of scientific colleagues (there were only four of them), he at the same time acquainted them with the results of the experimental research carried out in the other laboratories under his charge (at the Institute of Experimental Medicine and at the Physiological Chair of the Military Medical Academy).

This practice remained unchanged after the reorganization of the physiological laboratory into the Institute of Physiology in 1924, when the latter was accommodated in the premises it occupies now (Vasilievsky Island, Tuchkov Quay, No. 2A).

The numbers attending the "Wednesdays" grew steadily not only because of the increase in the staff of the Institute and the other Pavlov laboratories, but because of the presence sometimes of many physiologists and physicians who had received invitations.

Unfortunately, there are no records of the "Wednesdays" for the period from 1921 to 1929. From the end of 1929 until May 1933, V. K. Feodorov, one of the scientific workers in the Institute, regularly recorded the Wednesday meetings. Afterwards from the autumn of 1933 up to February 27, 1935, the day of Pavlov's death, these physiological discussions were taken down in shorthand. They are of great scientific value, revealing as they do the very process of Pavlov's scientific creative activity, his everyday "thinking" in close personal contact with numerous pupils and colleagues.

The present edition contains fragments of Pavlov's statements, mainly devoted to the interrelation of physiology and psychology, as well as to his tireless struggle against the idealistic concepts of some scientists abroad.

The stenographic records were edited in a manner that has preserved the peculiarity of Pavlov's expressions and turn of speech. The minutes of the Pavlovian "Wednesdays" were published in three volumes in 1949 by the U.S.S.R. Academy of Sciences (see "Pavlovian Wednesdays," Vols. I-III).

[163] R Yerkes—American scientist, author of numerous works on problems of general and comparative psychology, and especially the psychology of apes. He affirmed that the psychical processes in chimpanzees differ qualitatively from the associative higher nervous activity of other animals, while the difference between the mental activity of the chimpanzee and that of man is only quantitative. p. 551

[164] "Raphael" and "Rosa"—the chimpanzees used in Koltushi for experimentation with the aim of studying the higher nervous activity of anthropoids. This experimentation is now being carried on at the Pavlov Institute of Physiology of the U.S.S.R. Academy of Sciences in Pavlovo (Koltushi). p. 551

[165] See note 44 p. 554

[166] W. Koehler's book under the same title is implied here. It was translated into Russian in 1930. p. 558

[167] The book by Charles Sherrington *The Brain and Its Mechanism* was published in 1933. His next book *Man on His Nature* appeared in 1942; it deals with the problems of the history and philosophy of natural sciences. Basing himself on the idealistic concepts of

J. Fernel, 16th century physician and philosopher, Sherrington proclaimed the reactionary idea that the world is unknowable. p. 563

[168] Dubois-Raymond—well-known German physiologist of the 19th century. In his speech "Seven Enigmas of the World" he declared that the mysteries of mental life would never be disclosed by natural science. Since then the term "ignorabimus" has become the motto of all agnostics and avowed idealists. p. 564

[169] Richet Charles—outstanding French psychologist. He was professor of the Medical Faculty at the University of Paris and president of the Paris Biological Society. p. 566

[170] Spengler—reactionary German philosopher-idealist, one of the ideologists of German fascism. p. 568

[171] Gestalt psychology—reactionary trend in contemporary bourgeois psychology. According to the adherents of this trend, a psychical state constitutes an integral structure—a "gestalt," or "configuration." The latter cannot be decomposed into separate elements and is inaccessible to analysis, owing to which it cannot be made known. Koehler and Koffka, who head this trend, deny that behaviour consists of different reactions to these or other stimuli: The external situation and the reaction to it constitute a single structure which tends to a state of equilibrium. In this connection the adherents of the Gestalt theory reject the doctrine of the behaviourists, their theory of "trial and error," as well as the very principle of associationism (i.e., the formation of functional links between the sensations in the course of individual experience). The principles of the Gestalt theory, which affirms that mental activity is unknowable, and which, therefore, admits the existence of a particular, non-material and spiritual source, are applied by the adherents of this theory (for example, by Koffka) to all biological and even physical phenomena. They endeavour to prove that the latter represent definite structures, i.e., close integral processes, which cannot be decomposed into elements, since each part is fully determined by the whole to which it belongs.

In his statement Pavlov analysed the concepts of the Gestalt psychology and cited a book written by one of its adherents, the American Robert Woodworth, *Contemporary Schools of Psychology*, 1932; Pavlov subjected this idealist theory to annihilating criticism. p. 569

[172] The book by Kurt Koffka *The Growth of the Mind* published in 1924, is a translation from the 1921 German edition which appeared under the title *Die Grundlagen der psychischen Entwicklung...* p. 577

[173] S. V. Kleshchev. p. 579

[174] Pavlov has in mind the treatise by the English philosopher John Locke, "Essay on Human Understanding," written in 1687. Locke

denied the existence of inborn ideas and affirmed that all knowledge is acquired from experience. However, according to Locke, true knowledge originates not only from sensations, but also from another source—from reflection, i.e., the synthesis of sensations. p. 588

[175] Mary Becker-Eddie, who founded a reactionary religious current in the United States, the so-called "Christian Science." p. 590

[176] Maria Kapitonovna Petrova—a prominent Soviet scientist, Stalin Prize winner; one of Pavlov's closest co-workers. p. 592

[177] W. Koehler's book *Psychologische Probleme* was published in Berlin in 1933. p. 595

[178] F. P. Mayorov. p. 596

[179] Henri Bergson—reactionary French philosopher-idealist. According to Bergson's erroneous views, the methods of the natural sciences serve only as means of practical application, they do not elucidate the essence of phenomena; real knowledge of the world is effected through intuition. All the vital processes are governed by a vital torrent, or the so-called élan vital. There is a free indetermined connection between phenomena. Bergson strongly opposed the natural science theories of evolution and regarded evolution as a phenomenon of a psychical nature. p. 609

[180] Howard Warren—author of a number of reference books on psychology. In 1934 he published the "Dictionary of Psychology" mentioned by Pavlov. p. 613

[181] See note 83. p. 616

[182] Maria Kapitonovna Petrova. p. 618

[183] V. K. Feodorov. p. 618

[184] Sherrington's book *The Brain and Its Mechanism.* p. 620

[185] Ezras Asratovich Asratyan—pupil and colleague of Pavlov, Corresponding Member of the U.S.S.R. Academy of Sciences, Member of the Academy of Sciences of the Armenian Soviet Socialist Republic. p. 621

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